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Antiquity

A QUARTERLY REVIEW OF ARCHAEOLOGY

Edited by Glyn Daniel

Vol. XXXV No. 138 JUNE 1961

Editorial – – – – – – – – – – – 85
The Antiquity Trust – – – – – – – – – 90
DIMITER P. DIMITROV: *Seuthopolis* – – – – – – 91
SONIA COLE: *Forgeries and the British Museum* – – – – 103
CHARLES MCBURNEY: *Aspects of Palaeolithic Art* – – – – 107
HECTOR CATLING: *A New Bronze Sword from Cyprus* – – – 115
G. W. DIMBLEBY: *The Ancient Forest of Blackamore* – – – 123
P. ASHBEE & I. W. CORNWALL: *An Experiment in Field Archaeology* 129
L. R. PALMER: *The Find Places of the Knossos Tablets* – – – 135
Notes and News – – – – – – – – – – 142
Reviews (see overleaf) – – – – – – – – – 157
Book Chronicle – – – – – – – – 102, 122, 134

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REVIEWS

The Journal of Roman Studies, Vol. L (1960) (*Sir Mortimer Wheeler*) – – – – 157

Myrtou-Pighades. A Late Bronze-Age Sanctuary in Cyprus. By Joan du Plat Taylor *et al.* (*James R. Stewart*) – – – – – – – – – – – 159

The Egyptians. By Cyril Aldred. (*T. G. H. James*) – – – – – – – 159

Pervobytnaya Tekhnika (*Prehistoric Technology*) *By* S. A. Semenov (*Evzen Neustupny*) – 161

Sumer. By A. Parrot, *with a preface by* A. Malraux. (*J. M. Munn-Rankin*) – – – 163

Ur in Retrospect; in memory of Sir Leonard Woolley. IRAQ, Vol. XXII, 1960. *Edited by* M. E. L. Mallowan and D. J. Wiseman. (*J. M. Munn-Rankin*) – – – – – 164

A Matter of Time. By The Royal Commission on Historical Monuments (England). (*Sylvia Hallam*) – – – – – – – – – – – – 165

The Early Christians. By Michael Gough. (*W. H. C. Frend*) – – – – – – 166

Indian Life in the Upper Great Lakes 11,000 *B.C.–A.D.* 1800. *By* George Irving Quimby. (*G. H. S. Bushnell*) – – – – – – – – – – – – 168

Egypt of the Pharaohs, an Introduction. By Sir Alan Gardiner. (*Cyril Aldred*) – – – 169

EDITORIAL NOTICES

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Antiquity

VOL. XXXV No. 138 JUNE 1961

Editorial

IT is a pleasure to draw attention to two important, interesting and attractive Exhibitions in London this summer. The first is the Jubilee Exhibition of the Society for the Promotion of Roman Studies. It will be held at Goldsmiths' Hall (Foster Lane, E.C.2) from 27 June to 22 July and is entitled 'Art in Roman Britain'. It is the most comprehensive exhibition of Art in Roman Britain ever to be assembled, and, incidentally, the first to be held in the City of London. The exhibits are being loaned by over forty public authorities, museums and private owners and include the silver tray from Corbridge lent by His Grace the Duke of Northumberland, the brass helmets from Newstead lent by the National Museum of Antiquities of Scotland, the gilt-bronze head of Sulis Minerva lent by the City of Bath, and the Mithraic sculpture lent by the City of London. All the works shown in this exhibition were discovered in Britain and belong to the first four centuries of our era; they are selected to illustrate as fully as possible the many varied aspects of art that appeared in Britain under Roman rule—many are unique in the Roman world. Incidentally a complete photographic record of the Exhibition, with a general introduction and notes by Professor J. M. C. Toynbee, will be published early in 1962 by the Phaidon Press. May we remind readers of the address of the Society for the Promotion of Roman Studies; it is 31–34, *Gordon Square, London, W.C.*1.

The second exhibition is in the exhibition gallery of the Department of Prints and Drawings in the British Museum. It opened on 2 February and will remain open, we understand, for most of this year. It was designed to illustrate some aspects of forgery and deception in the fields of natural history, antiquity, coins and medals, the fine arts, literature, music and postage stamps. It is of course by no means the first such exhibition; one was held in London, at the Burlington Fine Arts Club, in 1924, another at the Ashmolean Museum, Oxford, in 1952, and a third in Paris, organized by that splendid body the *Comité des Salons Artistiques de la Police*, at the Grand Palais, in 1955. This is the first time that any such exhibition has been mounted in the British Museum, and it is discussed in an article in this present issue of ANTIQUITY by Mrs Sonia Cole, whose own book on fakes and forgeries (*Counterfeit*, London, 1955) makes such good reading.

A former distinguished member of the staff of the British Museum, when given a preview of some of the exhibits as they were being assembled, said 'what fun it is when someone takes an enormous amount of trouble to be really naughty', and in a recent B.B.C.

talk, Mr Cecil Gould, of the National Gallery, discussed the role of what he called 'superannuated schoolboy naughtiness' in the creation of fakes. There are some who have suggested that the Piltdown hoax started as such a piece of superannuated schoolboy naughtiness by a member of that fascinating group of young men led by Horace de Vere Cole who appeared here, there and everywhere—in Cambridge, in Portsmouth, in Piccadilly, variously disguised as the Shah of Persia, the Sultan of Zanzibar, or roadmenders. Mr Gould turned a pretty phrase when he said 'A fake only begins to be a fake when someone says it is genuine', and we were reminded of the American impressario who wanted to hire Rouchomowsky and the Tiara of Saitaphernes for a tour of American music halls, but only on condition that the French Government who had bought this piece of period jewellery in all good faith as antique, would give him a certificate that he was getting 'the genuine fake'. Mr Cecil Gould's talk has now been printed in *The Listener* for 6 April, 1961 under the title of 'The Ethics of Faking'. What were the ethics, and what the truth of the astonishing Piltdown affair? Is all yet known that can be known? Mr L. F. Salzman writes reminding us that this is the 50th anniversary of Charles Dawson's 'discovery' of Piltdown Man and says 'As 3 May is the "Feast of the Invention of the Holy Cross", I feel we might inaugurate a "Feast of the Invention of the unholy cross between Man and Monkey": April 1st seems a suitable date'.

Of course there are many reasons for forgery apart from schoolboy naughtiness, and while there is hope of monetary gain, and the existence of wealthy private collectors for whom the fact of ownership gives rise to passionate wilful thinking, we must be prepared for forgeries and deceptive copies in the whole field of art and archaeology from Palaeolithic Mural Art to Modern Painting, from Glozel to Van Meegeren (there is a 'genuine' Van Meegeren, the only one in England, and lent by the Courtauld Institute, in this exhibition).

We should mention here two recent books on forgery. The first is by Guy Isnard and is called *Faux et Imitations dans l'art* (two volumes, Paris, Librairie Arthème Fayard, 1959, 1250 francs each volume) and a translation of Frank Arnau's *Kunst der Fälscher der Kunst* (Dusseldorf, 1959) just published in Great Britain (see *Book Chronicle*, p. 134). Arnau has a particularly illuminating and fair account of Malskat's work at Schleswig and Lubeck, and of the Fey-Malskat picture 'factory' which, after the end of the Second World War, poured out Barlachs, Chagalls, Utrillos, and Henri Rousseaus. Arnau has no dedication to his book, but merely prints before his foreword these two sentences: 'According to the enlarged edition of his *oeuvre* catalogue, Corot painted over 2,000 pictures. Of these more than 5,000 are in the United States.'[1]

When, a few months ago, it looked for a short while as though ANTIQUITY might come to an end, as we described in our Editorial for March of this year, the present Editor found some slight solace in examining the history of British archaeological journals which had in the past set out to do the same things, or some of the same things, which Crawford achieved so well in ANTIQUITY between 1927 and 1957. If we exclude *The Gentleman's Magazine*, which for very long carried archaeological news and notes, the first journal in the melancholy chronicle of dead journals was *The Reliquary*, which began in 1860 under the editorship of Llewellyn Jewitt. *The Reliquary* had a splendid sub-title: 'A Depository for

[1] Cf. Sonia Cole's sentence, 'New York customs statistics revealed that over 103,000 Corots were imported from Europe over a period of twenty years during the present century! An eccentric old Frenchman bought up 2,000 of such' (*Counterfeit*, 13).

Precious Relics—Legendary, Biographical and Historical, illustrative of the habits, customs and pursuits of our forefathers.' Ten years later, in 1871, there was founded a new journal, *The Antiquary*, described as 'A fortnightly medium of inter-communication for Archaeologists, Antiquaries, Numismatists, the Virtuosi, and Collectors of Articles of Virtu and Curiosities'. It came to an end after a few years and was succeeded in 1880 by a new journal of the same name, usually referred to by bibliographers as 'Walford's *Antiquary*', which described itself as 'A Magazine devoted to the Study of the Past'.

Walford's *Antiquary* included in the preface of the first number the original prospectus, which said: 'In spite of the fact that this age lives so much in the present, worships progress so keenly, and looks forward to further progress so hopefully, there is in the breast of our "nation of shop-keepers" a deep-seated reverence for antiquity. . . . It is hoped that a Magazine devoted to the work of cherishing and fostering the antiquarian spirit in the various paths of inquiry and research, will meet with the support that it aspires to merit. *The Gentleman's Magazine* has for some time ceased to fill the position which *Sylvanus Urban* once held as the organ of all students of Antiquity. . . .' In 1894 there came into existence the nearest equivalent to a 19th-century version of ANTIQUITY, namely the journal called *The Illustrated Archaeologist*. It was edited by J. Romilly Allen and was sub-titled 'A Quarterly Journal devoted to the Study of the Antiquities of Great Britain: the Development of the Arts and Industries of Man in Past Ages, and the Survival of Ancient Usages and Appliances in the Present'—a sub-title very near to the heart and purpose of Crawford and of his generation of anthropogeographically trained or interested archaeologists. The first volume of *The Illustrated Archaeologist* had Articles, Notes, Notes on Books and Notes on Museums; the very first article was called 'A Very Ancient Industry' and dealt with the flint knappers of Brandon—and contained a very clear diagram of the complicated way in which the modern miners reached the flint veins. The second volume was equally attractively produced and included articles on Stonehenge, the Meayll Stone Circle in the Isle of Man, and the 1893 excavations at Silchester.

The Illustrated Archaeologist amalgamated with *The Reliquary*, and from 1891 onwards there appeared *The Reliquary and Illustrated Archaeologist*, which described itself as 'A Quarterly Journal and Review devoted to the Story of the Early Pagan and Christian Antiquities of Great Britain; Mediaeval Architecture and Ecclesiology; the Development of the Arts and Industries of Man in the Past Ages and the Survival of Ancient Usages and Appliances to the Present'—another very Crawford-like manifesto. *The Reliquary and Illustrated Archaeologist* was first edited by Romilly Allen and then, after his death in 1907, by the Reverend Dr J. Charles Cox, until it ceased publication with Volume XV in 1909. Walford's *Antiquary* went on until 1915 and then died.

The immediate post-war period when British archaeology at home was being reorganized and revitalized by Fox, Wheeler, Kendrick, Childe and Crawford, and when great discoveries were being made all over the world from Folsom to Mohenjodaro, Harappa and Yang Shao Ts'un, had no popular archaeological journal. There was of course the *Illustrated London News*, but no *Reliquary*, no *Illustrated Archaeologist*, no *Antiquary* to describe Colonel Hawley's excavations at Stonehenge, Campbell Thompson and Hall at Ur and Eridu, or Carter and Carnarvon's discovery of Tutankhamen's Tomb, or to review Crawford's *Man and his Past*, Burkitt's *Prehistory*, or Fox's *Archaeology of the Cambridge Region*. It might be thought in the mid-twenties that something was bound to start sooner

or later. So it did, in the form of *The Antiquarian Quarterly* in 1925, which, though describing itself as 'incorporating articles on archaeology and ancient art', was really a journal tied to the trade sale of antiques and produced by Messrs. Spinks. A second volume was produced in 1927, and then, it, too, died.

The year of the death of *The Antiquarian Quarterly* was the year of the birth of ANTIQUITY. Anyone who looks back through the volumes of the dead proto-Antiquities between 1860 and 1927 will see at once that Crawford modelled the format of his new journal on the earlier ones, although, unlike *The Reliquary and Illustrated Archaeologist* he did not use art paper throughout. Now why did ANTIQUITY succeed while the others had failed? And how did it manage to survive the 1939–45 war when Walford's *Antiquary* had been killed by the 1914–18 war? Was it the peculiar genius of its founder and first Editor? Or was it that by the late 'twenties there was at last an audience, sensed by Sir Bruce Ingram since 1900, for a clear and straightforward and immediate statement of the archaeological discoveries and the interpretation of history from them?—an audience which now supports *Archaeology*, *The Archaeological News Letter*, *Kuml*, *Skalk* and *L'Information archéologique*? It is too soon to say; after all, *The Reliquary* (if we count, as we must, its amalgamated period with *The Illustrated Archaeologist*) had a life of forty-seven years. ANTIQUITY itself is now in its thirty-fifth year, and has two more issues before it can claim to be as old as Walford's *Antiquary*, which died with its thirty-fifth volume, and twelve years to go before it equals the record of *The Reliquary*.

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But, apart from records, it is pleasant to be able to say now that, despite the upheavals of 6 December, 1960–late January 1961, ANTIQUITY continues with a larger circulation than ever before, and over half of which is outside the British Isles. We welcome our many new subscribers and tell them as we tell our old supporters, that the September and December numbers will contain, *inter alia*, Professor Masson's account of the new Russian excavations at Anau, Dr Beatrice Blance's study of the Chalcolithic Colonization of Iberia, an evaluation by Dr Geoffrey Bushnell of the new light which Carbon-14 dating has thrown on the pattern of American prehistory, Professor Richard Atkinson on 'Neolithic Engineering', and Dr Gustav Riehm on ' Salt Mining and Trade in Prehistory.' The new discoveries of Dr Louis Leakey at Olduvai (*Nature*, 25 Feb., 1961, 649; *The New Scientist*, 2 March, 1961, 534) have aroused widespread interest and will be discussed in our September number by Dr Weiner and Dr Napier in the light of the conference in London on 12–14 May, organized by the Prehistoric Society.

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We know only too well that the dislocation caused by changing publishing arrangements has meant some confusion to subscribers and a regrettable delay in dealing with administrative queries about subscriptions and back numbers. The Editor would be grateful if anyone who has a complaint or grievance about any requests for subscriptions, back numbers, indexes, etc., made in the last six months to which no satisfactory reply has been received would write direct to him or preferably to Heffers, 104, Hills Road, Cambridge, England, when they will be promptly dealt with. Will everyone now please note; no more correspondence to Mr Edwards of Ashmore Green, Newbury, Berkshire, and certainly no more to Dr Crawford at Nursling. (In the last week we received two books for review and three subscriptions addressed to Nursling.) And for those who are thinking of taking out

or renewing subscriptions, our publishing office is now *Heffers*, 104, *Hills Road*, *Cambridge*, and our Bank, *Barclays Bank*, *Bene't Street*, *Cambridge*.

One or two correspondents have criticized our use of in-text advertising. This was deliberate; no journal which is self-supporting can dispense with advertising, and advertisements in themselves are fascinating, at least when looked at after the passage of years. In turning over the pages of the many volumes of *The Reliquary*, *The Illustrated Archaeologist*, *The Antiquary* and other dead archaeological journals, we have been almost as fascinated with the advertisements as with anything else. Not only archaeological books were advertised, but sherry, armour, portable shelving, office-desks, paints, money-lenders, hop-tokens, sewing machines, cures for toothache, headache, and neuralgia (Hodge's *Sozodonta* . . . 'immediate and infallible'), sermon and manuscript paper, artificial teeth, Mr Nicholls' Patent *Volta Medical Cloth* (Simple! Natural! Effective! Greatly recommended by the Faculty), and Dr Carter Moffat's *Ammoniaphone* ('charged with a chemical compound combined so as to resemble in effect that which is produced by the SOFT BALMY air of the Italian peninsula when inhaled into the lungs—hence the term ARTIFICIAL ITALIAN AIR'). We cannot resist quoting the two finest examples we have found:

> *Anobium Eroditum*, the devouring book-worm, turns up its nose (and its toes) at Stickphast Paste.

and this remarkable entry from *The Antiquary* for 1871:

> Qualified Antiquaries, Archaeologists, Virtuosi, Numismatists, Clergymen, and Professors of Music or Arts aspiring to degrees of Foreign Universities may communicate with MEDICUS, 46, King Street, Jersey, who will send gratuitously instruction how to proceed.

This is obviously one of the addresses we must all keep in our special address books, like that of The Druid Universal Bond, which is 112, *Narbonne Avenue*, *London*, *S.W.*4.

Dr M. J. Aitken, of the Oxford University Research Laboratory for Archaeology and the History of Art, whose book *Physics and Archaeology* will be reviewed in the next number of ANTIQUITY, writes: 'We are keen to co-operate with field archaeologists who are excavating suitable kilns, furnaces and hearths etc. in Britain. If there is reliable evidence as to approximate whereabouts we are ready to assist in precise location by means of a proton magnetometer survey, but for reasons of economy and convenience, we are not keen to visit sites much more than a hundred miles from Oxford, except in special circumstances. . . . In considering the suitability of baked clay for the application of the technique of magnetic dating, the most important criterion is that the clay is still exactly in the position of its last firing. Secondly, several square feet of baked clay should be available (thickness is not so important, half an inch is enough) so that several 4 in. by 4 in. samples can be extracted. It is preferable to sample the floor of a structure, or as low down the walls as possible. Thirdly, the clay should be well burnt and not too friable to extract without crumbling. Sample-taking requires special apparatus and must be done by members of a laboratory skilled in this operation. It is necessarily destructive so that all photographing and drawing has to be finished first: satisfactory sampling of one structure usually takes a full day'. Dr Aitken adds that enquiries should be addressed to himself, or in his absence to Mr G. H. Weaver or Mr I. M. Cossar at 6 *Keble Road*, *Oxford* (Tel.: 55211).

The Antiquity Trust

IN the Editorial of the March number (ANTIQUITY, 1961, 1) reference was made to the new arrangements for the publication of this journal, and the vesting of its ownership in THE ANTIQUITY TRUST. The Trust, which has this year been created to purchase and own ANTIQUITY, was financed by generous gifts from the following individuals, institutions and trusts: Professor and Mrs R. J. C. Atkinson, Mrs C. M. Bennett, Mr John Baker (*Phoenix House*), Mr J. R. Boyden, Mr H. L. Bradfer-Lawrence, Mr J. W. Brailsford, Miss A. Brocklebank, Mrs O. P. F. Brogan, Mr M. C. Burkitt, Mr D. A. J. Buxton, Dr G. Caton-Thompson, Professor J. G. D. Clark, Mrs E. M. Clifford, the University of Chicago (the Department of Anthropology, and the Oriental Institute), Mrs Sonia Cole, Mr N. C. Cook, Cornell University, Mrs M. Aylwin Cotton, Mr J. D. Cowen, Mr H. R. Cox, Dr and Mrs Glyn Daniel, Mr Villiers David, Mr J. M. de Navarro, Mr Arthur S. Dewing, Mademoiselle S. de St.-Mathurin, the Hon. Robert Erskine, Dr Joan Evans, P.S.A., Dr E. G. M. Fletcher, M.P., Miss C. I. Fell, Mr S. S. Frere, Lady Fox, Sir Cyril Fox, Dr D. A. E. Garrod, Professor and Mrs W. F. Grimes, Mr Leslie Good, Dr D. H. Harden, Harvard University (the Peabody Museum, and the Fogg Art Museum), Messrs W. Heffer & Sons, Sir Francis Hill, Mr M. S. F. Hood, Sir Bruce Ingram (the *Illustrated London News*), Sir Thomas Kendrick, Dr Kathleen Kenyon, Sir Allen Lane (*Penguin Books*), Professor A. W. Lawrence, Mr Seton Lloyd, Professor M. E. L. Mallowan, Mr I. D. Margary, the Mediterranean Archaeological Trust, Mr Noel Morss, Mrs M. E. Murray-Threipland, Mr Walter Neurath (*Thames and Hudson*), Mr C. W. Phillips, the University Museum of Pennsylvania, Mr and Mrs J. B. Priestley, Professor Stuart Piggott, Princeton University (the Spears Fund), Mr H. J. Randall, the Russell Trust, Mr Harry Sacher, the Trustees of the R. J. Sainsbury Discretionary Settlement, Mr L. F. Salzman, *The Scientific American*, Mr Marcus J. Sieff, the University of Sydney, Miss M. V. Taylor, Mr J. T. Taylor, Toronto (the Royal Ontario Museum, and the University Library), Professor J. M. C. Toynbee, Lady Wheeler, Sir Mortimer Wheeler. This list is not complete because one or two donors have preferred to remain anonymous.

The Trustees of the Antiquity Trust are Professor R. J. C. Atkinson, Mrs M. Aylwin Cotton, Professor M. E. L. Mallowan, Mr I. D. Margary, Professor S. Piggott, and Sir Mortimer Wheeler. The solicitors are Messrs Few and Kester, Montagu House, Sussex Street, Cambridge, and the Bankers, Messrs Barclays Bank, Bene't Street, Cambridge.

Since the death of Dr O. G. S. Crawford in 1957 Antiquity Publications Ltd, a private limited liability company which owned and published ANTIQUITY, belonged to Mr and Mrs H. W. Edwards of Ashmore Green, Newbury. The primary purpose of the fund subscribed by the individuals, institutions, and trusts listed here was to acquire the whole of the issued share capital of the Company and thereafter to publish ANTIQUITY as a non-profit-making periodical. Clause 9 of the Draft Trust Deed of the Antiquity Trust says: 'The Trustees shall have as their primary object the publishing of ANTIQUITY as an archaeological review of the highest quality reasonably obtainable for the price at which it is published and shall not try to obtain any greater surplus in the Company of income over expenditure than is consistent with prudent management, reasonable provision for future liabilities and the desirability of making price changes as infrequently as possible'. Antiquity Publications Ltd continues as the trading company which produces ANTIQUITY. The Directors of this company are Dr Daniel and Dr Bushnell; the Secretary is Mr J. D. Tunnicliffe of Messrs Few and Kester, and the accountants Messrs Peters, Elworthy and Moore of Downing Street, Cambridge.

Seuthopolis

by DIMITER P. DIMITROV

Professor Dimiter P. Dimitrov, Curator of the National Museum of Archaeology in Sofia, was in charge of the excavations on the site of the Thracian city of Seuthopolis near the town of Kazanluk in Central Bulgaria. These were carried on from 1948 *to* 1954 *and revealed the details of the capital of Seuthes III, Ruler of the Odrysae. The present article is a brief report on the discoveries made there with special emphasis on the plan and architecture of the city. Professor Dimitrov gave a short preliminary report to the Conference of Soviet Archaeologists in Moscow in April* 1955 *which was published in Russian in* Sovetskaya archaeologia, I, 1957, 199–216.[1]

1. THE SITE

The site of the Thracian city of Seuthopolis lies about 8 km. to the west of Kazanluk, in the famous Valley of Roses, between the Balkan Range and the Sredna Gora Mountains. A spacious terraced area was formed by the erosion of the River Toundja (the ancient Thracian Tonsus)[2]; this is bounded to the north and east by a sloping plateau, on the site of the villages of Koprinka and Dounavtsi; and to the south, the steep slopes of the heights (site of the village of Morozovo (formerly Gorno Cherkovishté)), the last of the Sredna Gora foothills, dropped down to the river banks. The last and lowest step of this terraced area projects deep into the bends of the Toundja to the south, forming a peninsula, or tongue of land, the banks of which are 4 to 5 m. in height; to the west and south it is bounded by the River Toundja, and to the east the Golyama Varovitsa or Chiflikchiiska River, a small, but always swiftly flowing tributary of the Toundja, guards its approaches. The Thracian city of Seuthopolis was situated precisely on this spot, called 'Chiflika' (The Farm), on the left bank of the River Toundja, defended from the west, south and east by natural barriers of water (PLATE IX).

Up to the time digging began here in 1948, no one knew of this city's existence. Its name was not to be found in a single ancient source. Nevertheless, traces of a bygone culture were never entirely effaced either by time or people. Travellers through this part of the country in the 19th century were impressed by the large number of tumuli, which were massed in several groups along both banks of the river.[3]

[1] For certain finds and monuments from Seuthopolis, see Anna Aleksieva, 'Marques d'enseignes d'amphores de Koprinka', *Annuaire du Musée National Archéologique*, Plovdiv II, 1950, 185–190; M.Čičicova, 'Auftreten und Verwendung des Backsteins als Baumaterial bei den Thrakern von Ende des 4-ten bis Ende des 3-ten Jahrhunderts', *Bull. de l'Institut archéologique bulgare*, XXI, 1957, 129 f.; 'Timbre représentant des bijoux sur pithoi de Seuthopolis', *Studia in honorem academ. D. Decev*, 475 f. 'Les timbres sur Pithoi de Seuthopolis', *BCH*, LXXXII, 1958, II, 466 f.; D. P. Dimitrov, 'Neuentdeckte epigraphische Denkmäler über die Religion der Thraker in der frühhellenistischen Epoche', *Hommages à Waldemar Deonna*, Collection Latomus vol. XXVIII, 1957, 181–193, Pls. XXX–XXXI.

[2] E. Oberhummer, Tonsos, *RE*, 2te Reihe VI, 1714.

[3] F. v. Hochstetter, Über das Vorkommen alter Grabhügel in der europäischen Türkei, *Mtt. der anthropologischen Gesellschaft in Wien*, I, 1871, 97; F. Kanitz, *Donau-Bulgarien und der Balkan*, Leipzig, 1879, I, 235.

In 1939, Choudomir Chorbadjiiski, Curator of the Kazanluk Museum, and I visited this place in connexion with an archaeological map of the Kazanluk region, then in preparation; we were attracted by these tumuli, which were convincing proof of the presence of an ancient settlement here. There was a legend among the population that a monastery had once existed on this spot, and we were shown the place from which stones had been taken for building purposes. We were also shown coins, minted in the reigns of Philip II and Alexander the Great, and sherds of black-glazed Hellenic pottery were to be found in the fields. We also found that about 1 km. from the right bank of the Toundja, along the reaches of the small River Gyurlya, there were ruins of a medieval fortress on a hill; here iron arrow heads were found, and late antique coins. On the high crest in the direction of Morozovo (Gorno Cherkovishté) there were ten tumuli. On the high plateau to the north-east of the site there were another nine tumuli, and low down, next to the 'Chiflik' site itself there were three, which were as high as 9 to 10 m. It was clearly apparent that there had been intensive life here in very ancient times. This was also confirmed later by the discovery near Kazanluk of the Thracian tomb, renowned for its frescoes, which is about 7 or 8 km. away from this spot.

The construction of a large dam on this site, the Georgi Dimitrov Dam, furnished the occasion for excavations here in 1947; the reservoir which was to feed it, was to cover the entire area with its waters. That was why the Archaeological Institute and Museum at the Bulgarian Academy of Sciences in Sofia decided to begin archaeological studies on the site, and I was put in charge of them.[4]

Work began in 1948 and continued up to 1954. Two of the big tumuli[5] were excavated, in one of which, Tumulus No. 2, a cupola tomb built of baked bricks was found, which had, however, been destroyed in ancient times (PLATE XI (b)). Two later brick tombs had been built in its western part, with material taken from the original tomb, at a later date. A third big tumulus was excavated, in which another cupola tomb was found, also in ruins, but of particular importance, since it made it possible to elucidate the building technique of the Thracian cupolaed brick tombs.[6] As early as 1948 the boundaries of the settlement were established with its walls, towers and two city gates (FIG. 1). Work began within the settlement as early as 1950. From year to year the finds grew more varied and more interesting. Individual buildings, streets, wells, the sewerage system, and pottery, both Thracian and Greek imported pottery, were found in the very first excavations; there were numerous amphorae with stamps from the island of Thasos, pithoi with interesting potters' marks as well, fragments of sculpture, terra-cottas, household articles and so on. Finds of coins were particularly numerous. Over 2000 of them were discovered, of which 1300 were of the pre-Roman era, and the remainder of the 13th to 14th century A.D. Of the ancient coins the greater number, over 800, were minted by Seuthes III, the Thracian ruler, who was the leader of the Thracians in their liberation struggles against Alexander the Great and Lysimachus.[7] The remaining ancient coins cover a period ranging from Philip II (359–336 B.C.) to Demetrius II (239 to 229 B.C.). The chronological limits of the life of this settlement were fixed within this period.

Systematic archaeological research made it possible to establish the periods into which

[4] M. K. Čičikova, L. L. Ognenova and J. I. Cangova, research workers at the Bulgarian Academy of Sciences, took part in the excavations.

[5] Cf. K. I. Jouglev. 'Les fouilles et les recherches du tumulus No 1—Koprinka', *Annuaire de l'Université de Sofia, Faculté de Philosophie et d'Histoire*, XLVII, 1952, fasc. II, 217–289; XLIX, 1955, fasc. II, 35–194.

[6] M. Čičikova, *ibid.* 132 f.

[7] For Seuthes III see Swoboda, Seuthes, *RE*, 2te Reihe II, 2022–3.

Fig. 1. Plan of Seuthopolis.

the life of this ancient settlement may be divided. In the days of Philip II there was a small Thracian settlement on this spot, situated around a fortified residence (τύρσις), the remains of which produced coins struck by this monarch and Thracian pottery. A thin stratum of ashes separates the remains of this period from the upper cultural stratum. The latter shows that in the days of Seuthes III a fortified centre, laid out as a city, was built on the site of the former settlement, which had been destroyed by fire; the fortifications, the stone foundations of spacious buildings, the streets, the different articles of Greek and Thracian origin, coins of the reigns of Seuthes III, Alexander the Great and the Diadochi, found during the digging, all belong to this centre. After a siege, the city was taken by storm, pillaged and totally destroyed. The burnt mud-brick, the numerous piles of stones, paint an eloquent picture of the tragic fate which befell the city of Seuthes. The third period of its history, which continues up to the 3rd century B.C., marks an attempt to restore the ruined city. After that, life here was interrupted. About 20 Roman coins of the Imperial period were found, and 7 early Byzantine coins, accidentally dropped here while the land was being tilled after the disappearance of the ancient settlement. Only after a pause of about 1400 years did a small medieval Bulgarian settlement make its appearance, the inhabitants

of which lived in huts. From this settlement, and from burials in the eastern part of the ancient settlement, come medieval potsherds as well as a number of different implements and ornaments, and more than 800 medieval coins, mainly Byzantine.

Up to 1953, the name of the ancient city remained unknown. In that year a fortified quarter was found in its north-eastern part; the largest building, a palace-temple, was here, and in one of its rooms the first epigraphic monument in the settlement was discovered, which is furthermore the first official document issued by the Thracian rulers to be found in Bulgaria. It is a marble stele with a pediment, 63 cm. high, containing 37 lines of Greek text—a sworn contract between nobles of the Thracian and Macedonian ruling class. Quite apart from the tremendous significance of the entire text as a historical document on Thracian relations in the early Hellenistic period, (this will be considered elsewhere), the last few lines were of particular importance for us, as they mention the places where the four copies of this inscription were to be set up.[8] Two of these slabs were to be placed in *Καβύλη*, which town is known to us from ancient literature, and the ruins of which are being sought in the surroundings of the present-day town of Yambol in South Bulgaria.[9] Another city mentioned in the inscription is *Σευθόπολις*, without doubt the newly-discovered city, where one of the copies of the inscription has come to light. It was thus made clear that the second and richest ancient stratum of the newly-discovered settlement represented the ruins of this Thracian city, previously unknown to us.

This part of the inscription is also of particularly great significance because for the first time it provides data on certain buildings in both cities, which have radically changed our ideas about the Thracian city of this period in general. It is sufficient to compare the data given by Demosthenes in his speech (Demosth. VIII, 44)[10], delivered in 341, in which, among other cities (*Δρογγίλος* and *Μάστειρα*) Kabyle is mentioned, but as a miserable hole. In this new inscription, however, Kabyle is mentioned as a city with an *ἀγόρα*, in which there was an altar of Apollo. A *φωσφόριον* is also mentioned—a temple of Artemis-Hecate. The temple of the Great Gods (*Θεοὶ Μεγάλοι*) in Seuthopolis is also mentioned, and so is the altar of Dionysus in the agora. This information about Seuthopolis found in the inscription is supported by the finds, and by the city's plan and architecture, which will now be considered.

2. PLAN

Seuthopolis was a small city. It consisted of a fortified settlement, covering an area of about 12½ acres (FIG. 1), outside which, however, several other settlements existed to the north, the inhabitants of which were poor peasants, who lived in lightly built huts. The plan of the fortified city is marked by the wall, which forms an irregular pentagon. Two of its sides, the south- and north-western, form a right angle, while the remaining three form obtuse angles. With the exception of the north-western side, which bars the neck of the peninsula, the others follow in straight lines the banks of the peninsula formed by the rivers which surround it.

The fortified part of the city was built according to a plan which recalls the so-called 'grid-iron' system of Hippodamus of Miletus. As is known, this system appeared in the planning of Greek cities as early as the 5th century B.C.; however, it became typical of most cities in the early Hellenistic period, such as Priene, Miletus, Alexandria, Damascus,

[8] Cf. D. P. Dimitrov, *ibid.* 181 f.
[9] For Kabyle see *ibid.* 185 f. and the literature quoted there.
[10] Ed. E. A. Westermann, Berlin, 1902, 208.

Aleppo, etc.[11] In the north-west and the south-west fortress walls there are gates, and these two gates are the only ones of the city. Two streets, each 6 m. wide, lead from them to the centre of the city, where they intersect at right angles, bounding a large space to the west, almost a perfect square in shape, 46 m. × 48 m., and covering an area of 2208 sq. m. This was the city's agora, mentioned in the 'Seuthopolis' inscription. The level of this agora is 30 cm. lower than that of the surrounding streets. It is clearly delimited from the latter by a stone coping, traces of which were found at several places. The remaining streets are not so wide, 3 to 4 m. at most. They run parallel to the two main streets and intersect at almost right angles, forming rectangular plots for dwellings and other buildings. A street which encircles the entire city followed the fortress walls. All the streets, as well as the agora, were laid with small cobblestones.

At the northern end a quarter was discovered, with an area of 4620 sq. m., which was separated from the remaining parts of the city by a fortified wall. It had the form of a trapezium and was entered from the city by a gate through its south-eastern wall. Only one building was discovered within this area, as already mentioned; it was the largest of all the buildings found in the city, and was situated close to the north-west wall, symmetrically placed with regard to the axis of the entrance. The entire space between the entrance and the building was cobbled.

3. FORTIFICATIONS

The city wall has a total circumference of 890 m. It is 2 m. wide everywhere, except along the south, where it rises directly from the banks of the Toundja and is 1·80 m. wide. The pentagonal shape of the city was due exclusively to the nature of the terrain. All the walls, with the exception of one on the north-west, are built along the steep river banks, with the river forming a natural moat. There are no natural obstacles on the north-western side which faces the plain. No artificial moat was made here, but the wall was reinforced with special works. The north-west gate, placed exactly in the centre of this side, is built in the form of a square tower with two passages to the plain and the city. There were probably square towers at each corner of the walls. The foundations of the west, north and north-east walls are very well preserved. Barely perceptible traces of the south corner tower remain, and none of the tower of the east corner. On the north-west side two more intermediate square towers had been erected, placed at almost equal distances from the gate and the corner towers. However, the south-west side of the fortress, which follows the bank but is set back from it, was reinforced with bastions instead of towers, the wall being broadened by rectangular projections, 2 m. in width. Two of these bastions directly flanked the opening of the south-west gate, and the third was placed between the gate and the west corner tower. There were no intermediate towers and bastions on the remaining sides of the fortress, which bordered directly on the banks of the Toundja and its tributary.

The situation of the inner fortress is quite unusual. It is organically linked with the city's general plan, part of the north-west wall and the entire north-east wall of the city being utilized to this end, and defending it on the outside. Its south-west and south-east walls, however, were within the city. A very large tower on the south corner provided additional defence for the entrance, which took the form of an impressive propylon. There was also an intermediate tower on the south-west wall. It should be noted that this inner fortified

[11] *Vseobstaja istoria architekturi*, II, Part I, Moscow, 1949, 193; A. B. Bunin, *Istoria gradostroitelnogo iskusstva*, I, Moscow, 1953, 56; Fabricius, Hippodamus, *RE*, VIII, 1731 f.; M. Rostovtzeff, *Social and Economic History of the Hellenistic World*, 1941, Vol. II, 1051.

enclosure did not occupy the most convenient strategic position for a last defence from an outside attack. True, it was the highest point of the city, but there is only a difference of 1·50 m. between it and the lowest lying parts around the River Toundja. Taking the surrounding terrain into consideration, the best guarded spot was the east end of the city, where two sides of an inner fortress would always have been naturally defended by the steep banks, and the currents of the rivers. All the evidence points to the purpose of the inner fortress being mainly to defend the palace from internal surprises. It probably existed as a fortified palace, *τύρσις*, before the city was laid out, and was afterwards included in the general plan of the newly-founded city.

The fortifications were built of mud-brick and wood, upon low stone foundations. Preserved parts of them have the same even surface. The foundations were built of two rows of large rough-hewn stones, with their smoother side turned outwards. The lower of these rows projected slightly and formed an offset. The stones are usually of an irregular shape. Only at some places have they been hewn into almost square blocks, and at the north-west intermediate tower there are traces of rusticated masonry. The space between the facings is filled with quarried stones of varying sizes, cemented with mud. Mud-brick walls rose above the stone foundations, in a framework of wooden beams, nailed together with long iron nails, large numbers of which were found along the entire length of the walls. Wooden stairs led up to the breastworks of the walls and to some of the towers. The place in which two of these flights of stairs were situated is very clearly apparent in the stone projections on the inside next to the west and north corner towers.

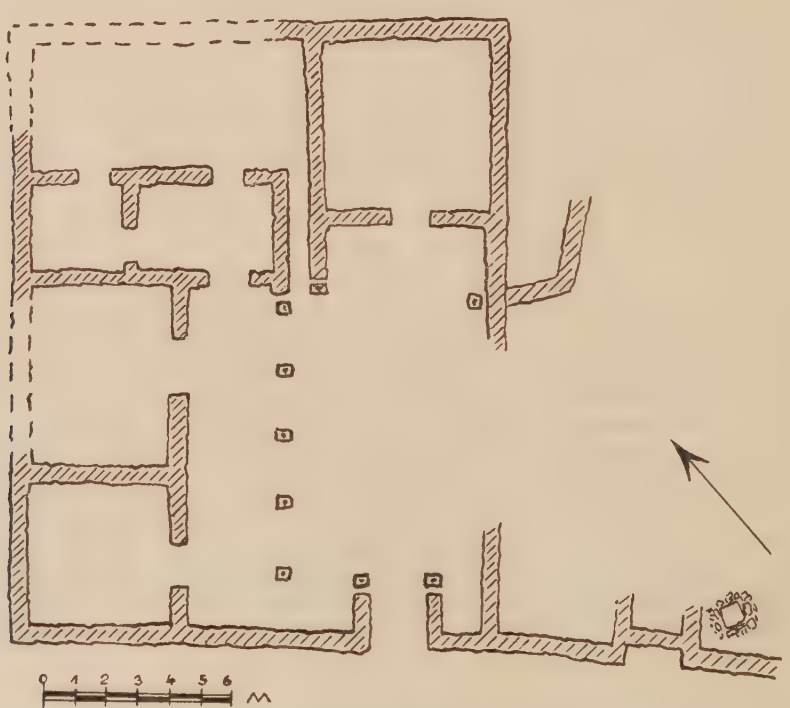

Fig. 2. Plan of the pastas *house.* (*See* PLATES X (*c*) *and* (*d*).)

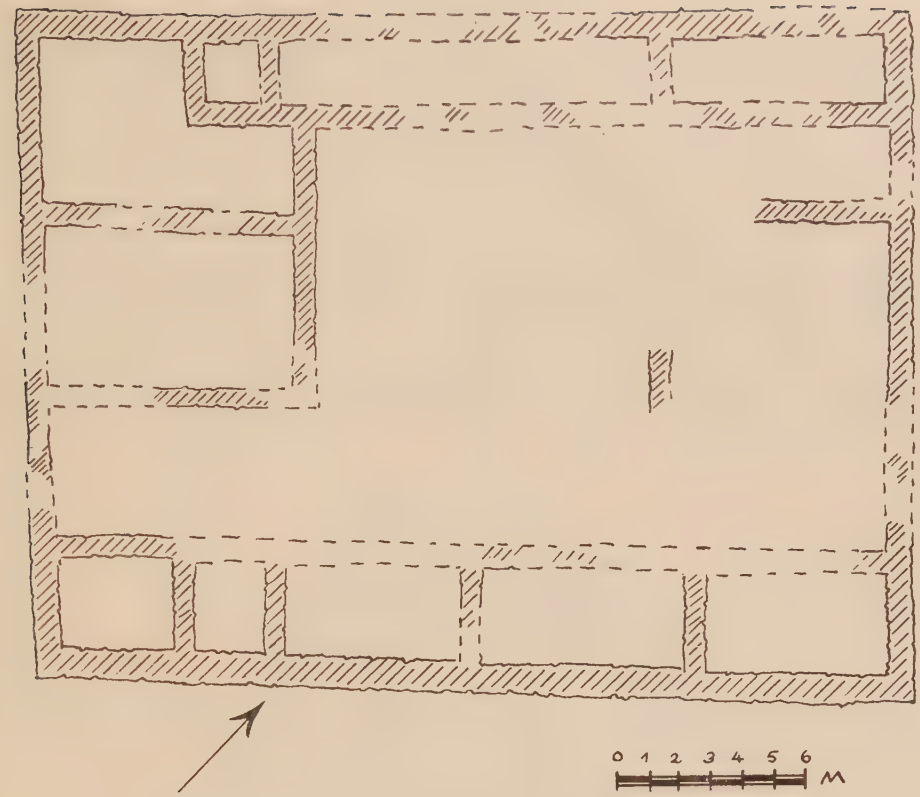

Fig. 3. Plan of the peristyle house.

4. THE HOUSES

The excavations at Seuthopolis give a good idea of the town houses in Thrace in the early Hellenistic period.

The residential quarters of the city are distinguished by the small surface they occupy, which corresponds to the small size of the town. The buildings themselves, however, which are few in number, are remarkable for their spaciousness. Each covered an average area of 300 to 350 sq. m. including the court and were usually placed in such a manner as to face on at least two streets. Thus, not more than two or three houses are found in one block. The houses of Seuthopolis had many rooms arranged around a central court, and some of them also had separate yards with outbuildings. The main living rooms were placed on the north-west side of the court, with their backs to the north winds, which blow down from the Balkan range; they were wide open on the south-east, facing the court.

The houses of Seuthopolis show a considerable variety in the details of their plans. Actually, a large part of the foundations of most of them has disappeared. This was particularly apparent in the east part of the city, where the medieval cemetery was. That is why it has not always been possible to reconstruct the plan of every house. At many places the existence of houses could be determined only by remains of the tiles or by the wells, of which something will be said later. Fortunately, however, in one house, the foundations of which were comparatively well preserved, we were able quite clearly to discover the plan of a *pastas* house (FIG. 2 and PLATE X (*b and c*)). The entrance was clearly marked on

the plan, leading from the street into a spacious court. On the north-west and the north-east lay the portico-'*pastas*', open to the south-east and the south-west with wooden pillars, standing upon square stone bases. Next to the entrance was the porter's room. The ground floor was occupied by store-rooms. The place where the staircase was, which led to the upper floor, is clearly visible; the living rooms were here, built around a covered balcony.

Another interesting house was discovered in the southern part of the city: its plan shows a rectangular court with rooms placed around it—a plan strongly reminiscent of the peristyle type of Greek house (FIG. 3). There was a separate yard along the river.

The houses of Seuthopolis were built on foundations of broken stone and clay. The superstructure was of timber and mud-brick. The roofing consisted of a wooden framework, covered with earth and plastered with clay, upon which wide tiles (*solenes*) were arranged, of the type familiar from Greek architecture. The joints were covered with half-curved tiles (*kalypteres*), which ended on the crest of the roof in antefixes, decorated with palmettes (PLATE XI (*d*)). In some of the buildings reed stems were also used for the ceiling and plastered with clay.

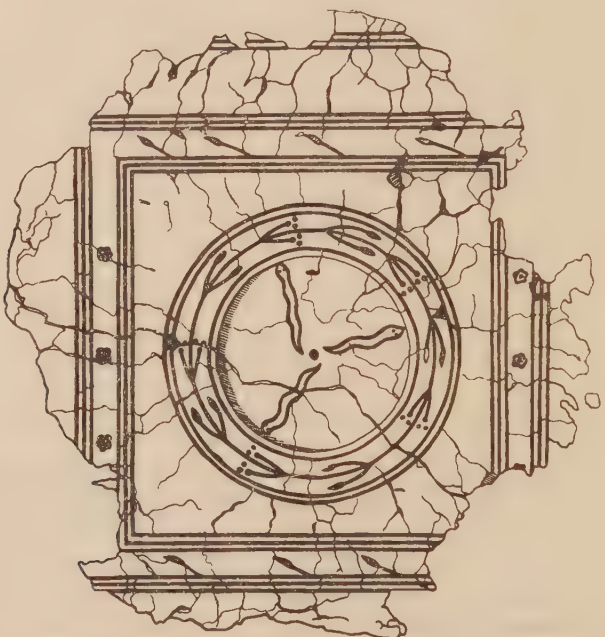

Fig. 4. Hearth-altar from Seuthopolis. (*See also* PLATE XI (*a*).)

Rammed clay, in many cases upon brickbats placed very close together, or potsherds, formed the floors of the rooms. In most of the rooms was a square platform made of clay, raised about 10 to 15 cm. from the floor, and almost always placed in the middle of the room (PLATE XI (*a*)); these platforms either had a flat surface or a circular depression in the centre. The interesting thing here was that in most cases these little platforms were richly decorated with incised ornaments. The richness and variety of ornamentation made a strong impression. Straight lines were predominant, grouped several at a time around concentric squares, and frequently surrounding concentric circles. In some cases the

squares were broken at the angles, and in other cases the square was outlined with a meander ornament. In many cases the bands thus formed were filled with plant ornaments: rosettes, leaves, undulating lines, ivy leaves or scattered rosettes. In the inside square of one little platform a small circle is depicted with four pairs of zig-zagging lines radiating from it, probably representing lightning. On another little platform three snakes are incised radially within the circle (FIG. 4). The position of these little platforms in the rooms, as

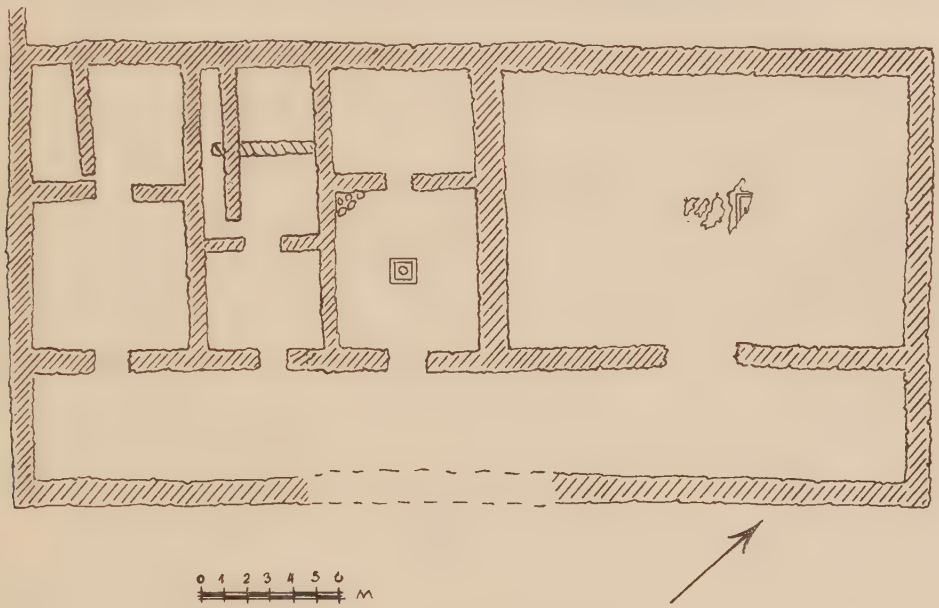

Fig. 5. Plan of the palace.

well as their structure, indicated that they were hearths. The symbolical figures were undoubtedly related to some kind of a domestic cult among the Thracians, in which the snake played an important part. At two places in the eastern part of the settlement coiled lead snakes were found. Similar hearth-altars have been found in Gaul, in the pre-Roman settlement of La Roque,[12] which also date back from the 4th to the 3rd century B.C.

The most important building in Seuthopolis was the one in the inner fortification—the *tyrsis*. Perhaps the parts of the only column found come from it: a Doric capital and the lowest drum, together with a square base. The building itself had an extremely interesting and original plan (FIG. 5). An antechamber, in the form of a corridor 40 m. long and 5 m. deep, running along the length of the building took one into the interior. This antechamber probably opened in front on to a colonnade. The interior is most irregularly arranged. Almost half of the area forms a spacious hall, 18 m. long and 12 m. broad: it is entered from the antechamber. The remaining half of the building is taken up by three double rooms, isolated one from the other, but each pair opening into the other and with another opening onto the common antechamber. In the inner section of the middle room there was

[12] See P. Larderet, 'L'oppidum préromain de La Roque, commune de Fabrègues (Hérault)', *Gallia* XV, 1957, fasc. 1, 25 f. Figs. 19, 21.

a staircase which led to the upper floor. The same building materials were used as in the other buildings of Seuthopolis. The crest of the roof was decorated with numerous ceramic antefixes, ornamented with palmettes.

This was undoubtedly the most monumental building in Seuthopolis, and the brilliance of its interior decoration was no less than that of its exterior. The remains of rich plastering were found in the great hall. Four layers of stucco were made to look like a marble revetment: a stone plinth imitating black marble ran round the bottom of the walls, and above this a narrow strip imitating multi-coloured marble, and the remainder was painted with Pompeian red. Wax, which was mixed in with the coloured marble dust, had given the stucco-work a glassy brilliance. In its spacious proportions and decoration this was indeed a ceremonial hall, in which the black plinth contrasted with the white imitation marble of the ceiling and the red walls. The hearth-altar was in the middle of the hall. A similar hearth, extremely well preserved, was found in one of the small rooms of this building too.

The unusual type of this building, its decorative character, as well as its special situation in the city plan, in a separate quarter, defended by its own fortified wall and tower, the imposing entrance and spacious and impressive hall, indicated that this must have been the personal residence of Seuthes III. The whole architectural entity has the character of a *tyrsis*, in the palace of which there was, besides the living quarters, a big hall, ἑστιατόριον known to us from the data given by Theopompus about Cotys.[13] But this large building also includes another element in its plan: the sanctuary of the Great Samothracian Gods, which might be expected in the double room next to the big hall, and which, according to the plan, resembled a temple with an opisthodome on the north side and an opening on the common antechamber, in which the pronaos was absorbed.[14] Moreover, as mentioned earlier, the inscription was discovered here. This architectural uniting of temple and residence of the Thracian ruler fully corresponds to the double function which the latter fulfilled—religious and political power were united in his person.[15]

The excavations revealed that Seuthopolis was not only a regularly planned city, as the Hellenic cities built from the 5th century on were, but that, like the contemporary Hellenic cities, it had both domestic and urban sewerage. In many of the houses drains were found consisting of clay pipes or of curved tiles (*kalypteres*), placed one over the other. They all led to common drains, which were mainly placed in the middle of the streets, and usually along those which ran from north to south, and sloped towards the south wall. At different places in the fortified wall here, there were openings with gutters, through which both the rain water and the sewage from the town ran out into the Toundja.

Water supplies for the city were drawn exclusively from wells. A well was found in the courtyard of every house; they were built of stones, and were either circular or rectangular in plan. One of the wells was built of bricks (PLATE XI (*c*)).

5. SUMMARY

The excavations made at Seuthopolis revealed new and hitherto unknown pages in the history of the material culture of the Thracians in the early Hellenistic period. It was found that they too had succeeded in developing their own urban culture which, as a rule, did not fall below the urban culture of the Hellenic world in many respects. Naturally, as in all

[13] Athen. 531–532; compare Kahrstedt in *RE*, XI, 1552; G. I. Kazarov, 'Die Ursprung und das erste Anblühen des Odrysenreiches im antiken Thrakien', *Ucilisten Pregled*, XXXII, 1933, 752.

[14] D. P. Dimitrov, *ibid.* 190; 'Zur Frage der thrakischen Religion in der hellenistischen Epoche', *Istoriceski Pregled*, XIII, fasc. 2, 1957, 73 f.

[15] G. I. Kazarov in *RE*, 2te Reihe VI, 550; Polyaen. VII, 22; Cass. Dio, LIV, 34.

aspects of the life of the Thracians, in the field of town planning and architecture, the strong influence of the architecture of the Hellenic world was noticeable particularly from the 4th century B.C. on. This is actually to be expected and it would be very remarkable were such an influence lacking, as Thracian society developed on the periphery of the classical world. Situated in the northern part of the Peninsula, far from the big southern sea routes, long isolated from the highly developed cultural centres of the Near East and the Aegean world, the Thracians lagged behind the tempo of development of the Hellenes in their own economic and social development in the first half of the 1st millennium. That is why, when they reached the threshold of the class society, the direct contact then established with the classical world through the Greek colonies of the Thracian shores enabled them to acquaint themselves with the more highly developed forms of Hellenic culture and to adopt those of its elements which corresponded with their new forms of life. Moreover, in most cases, these elements were creatively adapted to local conditions.

These phenomena were particularly clearly manifested in the architecture and town planning of Seuthopolis. Thus, for instance, in the plan of Seuthopolis the fundamental principles of town planning in the Greek cities of the classical and Hellenistic period were undoubtedly applied. But at the same time we should seek in vain for the closely packed insulae on the principle of grid construction in the newly created and reconstructed Greek towns, with their building plots initially equal in size, as is the case in Olynthus, for instance.[16] This democratic principle, which corresponded to the structure of Greek society, was lacking in Seuthopolis. Here, the insulae were not densely built over, the houses were detached, with considerable space between them, and took up building plots of different sizes, of which the largest was occupied by the palace. Undoubtedly, this was the reflection of a different type of society, one in which the unlimited power of a despot, who was assisted by the members of his dynasty and by the tribal princes, predominated. The situation of the palace quarter in Seuthopolis was most interesting. It looked as if it corresponded to the Acropolis in the Greek city. Actually this was not the acropolis of a Greek *polis* of the classical period, the religious centre of the city and the common refuge of the citizens in times of danger. This was first and foremost a *tyrsis*, the ruler's fortified palace, where he lived surrounded by his court. In this respect these elements in the plan of Seuthopolis remind one rather of the traditions of the town-planning system of the eastern despots.

Seuthopolis was a newly-founded city, built at one time according to a previously established plan, just as Alexander of Macedon and the Diadochi founded their new cities in the days of Seuthes III: the numerous Alexandrias, Antiochs, Cassandreias and so on. But Seuthes's city was founded immediately next to his *tyrsis*, which existed previously, and was included within the boundaries of the city as a special isolated quarter.

As we have said there was considerable variety in house construction in Seuthopolis. The *pastas* type of Greek house, predecessor of the peristyle house, was represented here in its purest form, but at the same time the *pastas* plan was in no way followed in the other houses. Other forms of houses appear of which, because they are not well preserved, and also because we know nothing of the Thracian houses of an earlier date, it is hard to say how far they continued the local tradition and how far they were borrowed from the architecture of other countries and peoples.

Borrowings from Greek architecture were most clearly apparent in the form of the roofing materials and in the inner decoration of the big buildings.

The broad tiles, known as *solenes*, were typically Greek in form. The *kalypteres* were

[16] Compare *Vseobstaja istoria architekturi*, 205.

actually semi-cylindrical, and not pentagonal, as they usually were in Greek architecture; however, semi-cylindrical *kalypteres* were not wholly alien to Greek architecture either. The decoration of the end *kalypteres* with antefixes, such as were found in large quantities in Seuthopolis, was entirely in the style of Greek architecture.

Polychrome stucco-work was used in the internal decoration of the rooms in certain of the larger buildings; it covered the mud-brick walls in imitation of marble revetment. This manner of decorating the walls of rooms, the incrustation style, was widely known in the Greek architecture of the day whence the Thracians undoubtedly adopted it.

Finally, the only column which was found, with the rectilinear profile of the echinus on its capital, was also a Greek Doric capital of the 4th to the 3rd century, in its purest form.

The technique of construction used in the architecture of Seuthopolis—mud-brick and wood upon a stone plinth—was widely known in the Greek construction of buildings. But it was also extensively known in the Near East and Central Europe.[17] The architecture of Troy II shows that this constructional technique was deeply rooted in the peripheral regions of the Aegean world, and in Seuthopolis it may represent a survival of these old local traditions.[18] However, what Seuthopolis does offer us as a novelty for the continent of Europe in building technique is the use, even though to quite a limited extent, of baked brick[19] in the cupola tombs. By their use of baked bricks as building materials in Seuthopolis at the end of the 4th century B.C., the Thracian builders were two centuries in advance of Rome, and in this respect may even have been in advance of the Greeks.

[17] Cf. W. Dehn, 'Die Heuneburg beim Talhof unweit Riedlingen', *Fundberichte aus Schwaben, N.F.* 14, 93 f.

[18] Compare the description of the tyrsis of Asidatus in Xenophon's *Anabasis*, VIII, 8, 12–15.

[19] For the baked bricks in Seuthopolis, see M. Čičikova, *ibid.* 129 f.

Book Chronicle

We include here books which have been received for review, or books of importance not received for review, of which we have recently been informed. We welcome information about books, particularly in languages other than English and American, of interest to readers of ANTIQUITY. *The listing of a book in this chronicle does not preclude its review in* ANTIQUITY.

PHYSICS AND ARCHAEOLOGY. *By* M. J. AITKEN. *pp.* 181, 28 *plates*, 50 *text figures. New York, Interscience Publishers Inc.*, 1961. $6. *London, Interscience Publishers Ltd.*, 43*s.*

THE ENGLISH FARMHOUSE AND COTTAGE. *By* M. W. BARLEY. *pp.* 297, 24 *plates*, 38 *text figures and* 8 *tables. London, Routledge and Kegan Paul*, 1961. 55*s.*

LES HOMMES PRÉHISTORIQUES ET LA RELIGION. *By* ETIENNE PATTE. *pp.* 194, 35 *text figures. Paris, Picard*, 1960. 10,18 *new francs.*

THE SAINTS OF CORNWALL. Part I: Saints of the Land's End District. *By* GILBERT H. DOBLE. *pp.* 145. *Printed for the Dean and Chapter of Truro by Parrett and Neves, Chatham*, 1960. 15*s.* The first volume of reprints of the lives of Cornish Saints written by Canon Doble (1880–1945), and now edited by Donald Attwater. To be obtained from *R. M. Catling, Pusey House, Oxford.*

[*continued on p.* 122.

Forgeries and the British Museum

by SONIA COLE

An Exhibition of Forgeries and Deceptive Copies was opened in the British Museum in February and is here discussed by Mrs Sonia Cole who has herself written a book on the fascinating subject of forgery, hoaxing and faking in archaeology and art-history (Counterfeit, 1955).

FOR its Exhibition of Forgeries and Deceptive Copies, on view in the Department of Prints and Drawings since February, the British Museum has produced many fascinating objects which normally it keeps hidden away: with the passage of time, it can afford to indulge its sense of humour. As Sir Thomas Kendrick is reputed to have said when viewing the exhibits: 'What fun it is when someone takes an immense amount of trouble to be really naughty.'

Contributions from the various Departments and from borrowed sources include Antiquities of all kinds, Ethnographical material, drawings and other examples of the Graphic Arts, Manuscripts and Printed Books, Music, Coins and Medals, and Postage Stamps. The Natural History Museum has also contributed a very popular section.

The only criticism of this entertaining and enlightening venture is that it would have been improved by concentrating on Forgeries and leaving out the Deceptive Copies. Few of us, except the specialist, can avoid boredom when confronted with rows of water-colours after Turner, or drawings after Canaletto. Works of the innocent copyist, or the conscientious restorer, even though exploited later by unscrupulous dealers, are in quite a different category to the productions of any forger worthy of the name. We may marvel at the technical skill of copyists, but it is the original and imaginative fakes that are interesting.

MASS-PRODUCED FORGERIES

Although forgery has been profitable for hundreds and even thousands of years, it was particularly lucrative during the 18th and 19th centuries, when rich patrons created an unprecedented demand for antiquities and for every imaginable object which enjoyed a certain prestige and snob value. Today collectors are still more numerous, often even richer, and probably more snobbish than ever; but, because they now consult the expert before parting with their cash, the lot of the modern forger is far harder.

During the 19th century, mass-produced Egyptian scarabs and ushabti figures were readily accepted by gullible tourists. Tanagra figures of the 4th and 3rd centuries B.C. were imitated and sold in immense numbers soon after their discovery in the 1870s, though today even the most untrained eye can pick out the Victorian sentimentality of the fakes. Among the most numerous, and the most naïve, of 19th-century forgeries are 'Billies and Charlies', rather charming little lead figures of which some 2000 were planted in excavations for the new Shadwell dock by two illiterate workmen. They are still dug up in many parts of the country today and haunt the B.M. to such an extent that it has a duplicated information sheet on 'Billies and Charlies' to hand to hopeful enquirers.

Classical subjects enjoyed an immense popularity from Renaissance times onwards and

16th- to 18th- century fakes are now valued as works of art of those periods. Some of the 'Classical' sculpture produced *ad nauseam* during the last century is very fine, as the celebrated bust of Julius Caesar bought by the B.M. in 1818, reproduced in countless history books as an authentic likeness but not even based on a contemporary portrait.

As tastes changed and clients became more discerning, forgers of course kept up with the times. Modern imitations of Oriental antiquities are often very fine; an example is the Siamese Buddha, in the style of an 11th–12th century Khmer head illustrated in PLATE XIII (*a*).

The exorbitant prices paid for African masks and wooden figures, and particularly for bronzes in the style of Ife and Benin, have provided a powerful incentive to European as well as African forgers. In the exhibition are two bronze copies of Bakongo wooden figures which might well deceive the eye; but bronze, though commonly used in Nigeria, is rare in the Congo and in addition the figures have been cast by the modern European sandbox technique instead of by the lost wax process which is universal in Africa.

Similarly the high prices paid for American antiquities, plus stringent laws which prevent the export of genuine objects, have encouraged both smuggling and forging. It has been estimated that 5000 first-class forgeries are made every year in Mexico. In the exhibition is a group of Mochica style pottery vases from Peru which were undoubtedly all cast from the same mould, probably a genuine one. One was presented to the B.M. in 1907, another was bought in 1937 and a third was presented in 1944; a fourth has been in the Musée de L'Homme since the end of the last century. A two headed 'Eagle' Knight vase in the British Museum is similar in style, though not from the same mould. PLATE XII (*a*) is a forgery of pure imagination, and apparently uninfluenced by any original. It represents a Colombian pottery effigy vase—and may well be one of the finest forgeries to come from Colombia.

THE GIANTS: IMAGINATIVE AND CREATIVE FORGERS

Apart from the multitude of forgers whose sole motive is financial gain, there are those who set out to prove theories, or to get their revenge on critics and experts, while the objective of many is to gain notoriety. It is among this group that the creative artist is found and even the occasional genius.

One of these was Thomas Chatterton, the boy poet whose works live under the name of Rowley, an imaginary 15-century monk of Bristol. Chatterton committed suicide at the age of 18 (the suicide rate among forgers is high) but his *Rolle of Sayncte Bartlemeweis Priorie* has an honoured place in literature. Nor should we forget James Macpherson, 'translator' —in actual fact creator—of the Gaelic poems of Ossian. Less talented was William Henry Ireland whose play *Vortigern*, alleged to be a new discovery of a work by Shakespeare, was killed by laughter at its first and only performance at Drury Lane in 1796. Nevertheless, some of his Shakespearean fakes caused Boswell to fall on his knees and declare 'I now kiss the relics of our bard and give thanks to God that I have lived to see them.'

Most celebrated of all literary frauds was Thomas Wise, creator and marketer of falsely dated pamphlets purporting to be first editions. His most sensational product was the '1847 Reading edition' of Elizabeth Barrett Browning's *Sonnets from the Portuguese*, dated three years earlier than the previously known first edition. Carter and Pollard's brilliant investigations in 1934 showed that Wise was careless in technique: some of his pamphlets, for instance, were dated earlier than the first appearance of the paper on which they were printed (the '1847' edition of the *Sonnets* was on chemical wood paper, first imported to this country from Sweden in 1874). In 1956, nineteen years after Wise's death,

it was discovered that he had stolen over 200 leaves from British Museum copies of 17th-century plays, which he used to complete imperfect copies in his own library and in those of his clients.

Among the manuscripts is a Chinese oracle bone showing true and false characters, side by side. Used for divination during the 2nd millennium B.C., the findings of the oracle were recorded in archaic script on the bone itself. The smaller characters near one edge are genuine, the larger ones beside them were probably added by dealers in Anyang about 1920 (PLATE XIII (*b*)). Of particular interest is the Shapira manuscript, offered to the B.M. in 1883 by a Polish dealer living in Jerusalem. It was a text of the book of Deuteronomy, written on leather in archaic Hebrew characters, with variations from any previously known texts. Shapira said he obtained it from a Bedouin, who had found it in a cave in Moab. Soon after, a French scholar declared it to be a forgery. Shapira committed suicide and the manuscript disappeared (a facsimile is on view in the exhibition). Since the discovery of the Dead Sea Scrolls, it has been alleged that the manuscript was genuine, but most authorities do not agree. One wonders whether Dead Sea Scrolls are being faked at this moment?

In the section on the Graphic Arts, the works of 'Captain John Eyre' take pride of place. This character, said to have been born in 1604, was the invention of one Peter Thompson of Regent's Park who lived in the middle of the 19th century. Thompson had a fairly good knowledge of architecture and the drawings mostly depict ancient buildings in and around London, executed on old paper with ink apparently faded by time.

Han van Meegeren, the greatest art faker of modern times, is represented at the exhibition by the only example of his work in England, loaned by the Courtauld Institute. It is a copy of a picture by the Dutch painter van Baburen, which van Meegeren meant to pass off as a copy made by Vermeer.

NATURAL HISTORY

The Natural History section contains such bizarre creatures as composite mermaids, part monkey and part fish, with wire spine and ribs revealed by the prosaic eye of the X-ray camera (if you long to own a mermaid, you can still buy one in Aden or Japan) (PLATE XIV). Other objects are genuine enough in themselves, but the credulous are persuaded to believe that they are something else, or that they possess magic powers. A narwhal's tusk becomes a unicorn's horn, or an ibex horn a griffin's claw. Roots of the mandrake (the English bryony) were much in demand as love philtres, opiates and aids towards pregnancy. In Germany these forked roots, so like human limbs, were fashioned into a homunculus, prized for its oracular powers. The Lamb of Tartary, actually the rhizome of the fern *Cibotium barometz*, has a downy coat which was supposed to be a cure for spitting blood; it was exhibited by Sir Hans Sloane at the Royal Society in 1698.

Casts of 'fossils' planted by students of Dr Beringer around Würzburg early in the 18th century seem to us wildly improbable, but they completely deceived the unfortunate Professor and this hoax illustrates the abysmal ignorance of palaeontology which prevailed until quite recent times.

The star of the collection is, of course, Piltdown Man, the subject of what must be a record number of tests by elaborate techniques. Anthropology is an even younger science than palaeontology, but the wealth of recent finds and the detailed examination they receive makes it most improbable that anyone would get away with another Piltdown.

Some day the Natural History Museum should stage an exhibition of Fakes and Forgeries of its own. It would include, of course, photographs of the Loch Ness monster and a copy

of the yeti's scalp. Another candidate would be the 'midwife toad' (*Alytes obstetricans*) whose nuptial pads were injected with black ink by Kammerer in an attempt to prove Lamarckism. The modern equivalents of mandrake roots should also find a place: rhinoceros horn traded for aphrodisiacs; and von Koenigswald's famous *Gigantopithecus* teeth sold with 'dragon's bones' in Chinese drugstores for medicinal purposes.

DETECTION

Keepers have learnt by long experience—and a good many awful warnings—to nose out a fake almost at a glance, but nearly every object they think of buying is submitted to the Research Laboratory just to make sure. Although it must be very seldom—if ever—that a new forgery finds its way into the collections, old ones still turn up which were never suspected until they happened to be examined particularly closely, perhaps when they were due for repair or cleaning.

A Bacchus cup of Roman style from Egypt, for instance, was discovered to be bogus only when it was cleaned recently. The colour of the metal aroused suspicion and an internal liner joining the two halves had no business to be there. It was found that the base had been spun from brass sheet and electroplated with copper. The Bacchus heads on either side of the cup are electrotypes (PLATE XII (*b*)).

Although the vast majority of forgeries can be condemned on style, or by the technique of manufacture, a small proportion may have to undergo scientific tests. The first step, of course, is to look at the object under a microscope. X-ray photography, or photography under the electron microscope, generally provides infallible proof of an object's *bona fides*. As a last resort, X-ray diffraction may be necessary. Microchemical tests, involving the sacrifice of only a minute sample from the specimen, are very often made. Later repairs or restorations, invisible by ordinary light, show up by their different fluorescence under ultra-violet rays from the mercury vapour lamp.

Usually it is not difficult to spot a forgery if you are looking for it, but many must have escaped detection because there was no reason to suspect them. Being wise after the event, it seems astonishing that a few flamboyant examples among the exhibits have been debunked only quite recently. The famed sarcophagus, bought as Etruscan in 1873 but in fact made in Rome in 1860, was not relegated to the cellars of the B.M. until 1936, although the inscription on the lid is taken from a brooch in the Louvre and reliefs of the banquet scene contain utensils of all periods from the 7th to the 5th centuries B.C. (PLATE XV (*a*)).

Soon after the B.M. exhibition opened, the Metropolitan Museum of New York announced that their three large and well-known figures of Etruscan warriors were fakes. Professor M. Pallottino, the Etruscan scholar, then cast aspersions on the St Louis Museum's terracotta Diana, whose virtue has been ardently defended by the museum's director. Other curators are no doubt examining their Etruscan collections with some anxiety.

The B.M. can afford to be smug when it remembers that the Louvre paid 200,000 francs for the gold tiara of Saitaphernes, handiwork of the talented Rouchomowsky of Odessa; or that in 1909 the Kaiser Friedrich Museum of Berlin bought for £8000 a bust of Flora believed to be by Leonardo, but in fact made by Richard Cockle Lucas of Romsey from candle-ends and stuffed with a piece of curtain.

The modern demand for scientific proof has shattered many cherished illusions: during tests made in 1934 on wheat from ancient Egyptian tombs, for instance, no case of germination was recorded, but this popular legend is hard to kill. It would indeed be dull if we could solve every mystery and happily we still have our Abominable Snowmen, our Loch Ness Monsters, and our Rouffignacs.

Aspects of Palaeolithic Art

by CHARLES McBURNEY

An English edition of Professor Graziosi's monumental survey of Upper Palaeolithic art has recently been published (Palaeolithic Art. *By Paolo Graziosi. pp. xi,* 278, 306 *plates,* 41 *figures,* 2 *maps. London, Faber and Faber,* 1960 £12 12*s.* 0*d.*) *Dr C. B. M. McBurney of the Department of Archaeology and Anthropology in the University of Cambridge, in reviewing this book, takes the opportunity to range widely over some aspects of man's oldest art.*

IT is a curious reflection that although palaeolithic art has been known and recognized as such for over a century, and despite the fact that during this time an enormous volume of literature has accumulated on the subject, yet the publications consist almost exclusively of articles dealing with limited aspects. These comprise descriptions of individual finds, or particular sites, but there are virtually no works on any scale to present the art as a whole in its so-called 'mobiliary' manifestations (that is to say, the decoration of loose and individual objects), together with the decorations found on the walls of caves. There have of course been many contributions of significance to certain general issues, such as chronology and correlation with the cultural succession; among the most important early works on the mobiliary art for instance is Piette's *L'art pendant l'âge du renne* of 1906. Again, in his magnificent pioneering studies on the sites of Altamira, Font de Gaume, and Les Combarelles, the Abbé Breuil devoted a number of chapters to general discussion of fundamental importance; many of these deal with all sides of the evidence in connexion with different problems, such as representation of different types of animals. But in no case has the archaeological public been rewarded with a full-scale work on the art as a whole by the greatest living authority. Although in his introduction the author of the work under review disclaims any intention to provide either a treatise or a complete corpus of the artistic achievements of the Palaeolithic, and describes his work rather as a 'panoramic view of that prodigious phenomenon which is the birth and development of the most ancient art in the history of man', yet he has in fact gone some considerable way to fill this important gap.

The book in its Italian edition is one of a series of general works dealing with the history of art. Although all are of a general character and designed for the artistic rather than the strictly archaeological public, they are clearly intended none the less to be of a thoroughly scholarly character. The present work is no exception and the author is accordingly obliged in his introduction to deal not merely with the art itself but with its context in the prehistoric successions of South-West Europe. At the time of writing this is no light undertaking, since the broad outlines of the Upper Palaeolithic succession of South-West Europe, which for so long seemed a stable structure likely to undergo only relatively minor additions and modifications, owing to the evergrowing volume of current research are in fact much in need of review to bring greater precision to the general perspective and also to correct not a few particular episodes. There are moreover many gaps in the evidence which require to be recognized and correctly defined even in a specialist work of this kind.

Among the factors which make such adjustment inevitable one might cite two in particular. First there is the acquisition (largely since the author prepared his text) of an absolute time scale for the period. This is of course due to the progress of C14 dates, and the importance of the evidence available will be sufficiently obvious to all who have read Professor Movius's recent epoch-making paper on the subject,[1] which appeared within a few months of the work under review. There can be no doubt that many of the problems that the author was obliged to treat as open or even to skirt as too controversial are now in large measure settled. A second development of the utmost importance of which the full significance was hardly available at the time of the preparation of Professor Graziosi's text was that of the full amplitude and precision of the current Russian researches in the Upper Palaeolithic. From a consideration of these researches, now rapidly becoming available to the Western archaeological public, it is clear that the Russian school of prehistorians has long developed and relied on schemes regarding the Upper Palaeolithic succession which are totally different from those of the current French prehistoric school. That essential modifications and developments of both schools of thought are required is painfully obvious; although no serious attempt has so far been made to combine the two into a convincing synthesis, such an attempt cannot now be long delayed. When it comes it will certainly mean that the all-important background of human movement and definition of societies in space and time, as it effects the artistic achievements, will have to be redrawn in earnest. With regard to the book under review, however, it should be added that the relatively restricted attempt of the author to provide an interpretative background means that the greater part of his text will remain usable, if inaccurate in detail, for a considerable period.

There are moreover a number of general problems of wider interest to which an answer can hardly be expected in the very near future. As an episode in human history Palaeolithic art is unique in a number of significant respects. It is unique in that it supplies us with our first concrete indication of the emergence of conceptual thought as we know that process from personal experience. For the same reason it is unique in our conception of the history of art, and that not merely because it comes nearer to supplying us with a vision of the original form of this distinctively human type of activity, but because it is probably the highest achievement in this field ever reached by a society of a purely hunting and nomadic kind. Finally; it is also of special significance in a way not divined by some of the earlier workers on the subject (but correctly guessed at by the Abbé Breuil) in that it represents the longest single pattern of human tradition of comparable complexity continuously transmitted throughout any society or group of societies known to science.

In all these respects Palaeolithic art is likely to provide a fruitful field of enquiry for historians and historical philosophers for many years to come. That it has not done so with more striking results so far is probably due to the elements of uncertainty regarding its age, cultural correlations, distribution in space and time, and finally and above all the course of its stylistic development. Some of these elements of uncertainty have no doubt been exaggerated for a number of years by researchers not immediately concerned in the field. These have regarded with suspicion the conflicting theories on many such points that have been propounded from time to time, particularly those regarding the stylistic development of the different branches of the art and the degree of variation or tolerance to be regarded as probable within any single phase or stage of development. It is one of the merits of the present work that it does in fact succeed in delimiting such important aspects of the problem as the spatial distribution of the art, its stylistic range, the range of its content, and a number of other intrinsic features.

[1] Hallam Movius 'C14 dates . . . ', *Current Anthrop.*, October 1960.

Probably the most fundamental single philosophical problem offered by Palaeolithic art lies in its implications for the general problem of the emergence of art as such. Those who have read this book may rest assured that they have in it a reliable picture of the basic evidence bearing on this question. The two great distribution maps provided, of the mobiliary art and the cave art, are likely to be regarded as corner stones of research for many years to come, since they establish convincingly the limits of geographical distribution. Archaeologically it has long been known that there is not the slightest trace of artistic activity even in the form of the most rudimentary pattern before the beginning of the Upper Palaeolithic cycle of cultures in Europe. Culture phases which can be regarded as the equivalent of the Upper Palaeolithic in Europe have now been adequately established in South-West Asia and in eastern North Africa and there can now be no question of their extreme poverty at least in art. Outside these limits we have positive evidence of the continuance of the earlier Middle Palaeolithic phase which both there and in Europe is totally devoid of any signs of artistic activity.

In Europe itself the earliest traces of artistic activity are not in fact exactly coterminous with the Upper Palaeolithic. In south-west Europe and the Mediterranean indeed industrial material auxiliary to the stone tools, in the form of carved bone for purely functional purposes, does in fact provide some evidence from the very first stage of the sort of conceptual thought which characterizes the corresponding elements in later stages of the Upper Palaeolithic. But actual works of art have not so far been associated with this initial phase.

In France, Central Europe, the Balkans, and Northern and Central Italy the immediately ensuing (and much more consistently characterized) cultural episode generally known as the Aurignacian does however show, sporadically and in different areas, fairly widespread signs of intentional decoration of objects by incised patterns of strokes and the like, and in south-west France a few rare cases of a remarkably primitive and rudimentary form of representational art are found. At the site of La Ferrassie in south west France we have the most complete traces of this kind comprising painted and engraved slabs with barely recognizable animal designs together with undoubted use of symbols conveying the notion of the female genitalia. Remains of similar age and character occur in the Pyrenees, but are notably lacking in Central and Eastern Europe. There is however one apparent exception in that area to this rule, namely, the famous animal figurines from the cave of the Vogelherd in Bavaria. These are, however, so remarkably similar to the products of the next cultural phase, namely, the East Gravettian, in this area that the great majority of workers regard them either as intrusive in the stratigraphical sense from layers of this later period, or else as due to acculturation between a surviving Aurignacian and an early Gravettian. It would indeed appear that another and still more famous group of productions formally attributed to the Aurignacian in south-west France, namely, the 'Venus' figurines, which form one of the most striking and significant features in the whole field of the study of prehistoric art, must now be ascribed to one or other of these causes.

The real beginning in the study of prehistoric art from the chronological point of view lies not so much in the crude initial indications proper to the Aurignacian, but throughout the subsequent Gravettian (in South-West France 'Perigordian' *auct.*) period. From the point of view of absolute chronology the earliest dated deposits of this tradition in south-west France—though not necessarily the earliest of their kind—are dated to *circa* 22,000 B.C.; in Central Europe however a series of dates in the region of 24,000 can now be regarded confidently as delimiting the main efflorescent of the corresponding finds in that area.

At the other end of the scale the date of the *end* of the Upper Palaeolithic art cycle, namely, that associated with the Magdalenian culture in South-West France, we may place with

confidence at least as late as the 11th millennium B.C. and possibly as late as the 9th. Thus the evidence of Carbon-14 is now sufficient to assure us beyond reasonable doubt that Palaeolithic art represents a continuous tradition unbroken for a period of 13,000 to 15,000 years. While this figure is more modest than the '400 centuries' of the Abbé Breuil, it is none the less a staggering one and, as already mentioned, demonstrates the existence of a human tradition for a longer period than any other of which we have positive knowledge.

The problems of wider import raised by this astonishing observation alone are many and various. The literature of art history is in the main concerned with periods and societies of a totally different character. During much of the 19th century for instance attention was concentrated on the classical Greek and Roman productions or those of the Middle Ages and Renaissance. Subsequently careful scholarship was devoted to the studies of Far Eastern traditions such as the Chinese and the Near Eastern traditions in Mesopotamia and Egypt. The repetition, or apparent repetition, in most of these traditions of certain types of episode as for instance 'Archaic stylization', followed perhaps by the increasing 'realism' and a final period of so-called decadence or idealization ending in still further retreat from naturalism tended to favour certain preconceptions regarding the nature of artistic development in general. Probably it was not until the turn of the century that such works as Haddon's and Balfour's studies of primitive art and Haddon's analysis of art in New Guinea that a serious start on the study of the artistic traditions of people in an entirely different state of culture was made. It was about this time also that the profound difference in the intellectual patterns of societies dependent on food gathering, rather than food production, for their subsistence began to be fully realized by cultural historians. Thus gradually was the magnitude of the problem presented by Palaeolithic art realized. In the early works of Breuil we can see two influences at work, those deriving their inspiration from the 'cyclical' concepts of the earlier generation of art historians, and those based on the direct study of more primitive peoples as represented above all by Haddon's work in the New Guineas.

It is probably true to say that even today we are still very imperfectly equipped to define and solve many of the major philosophical problems posed. Yet some general aspects are beginning to take shape. Perhaps the first of these in importance concerns the nature of the very earliest surviving works and their relationship to the tradition as a whole. Here the author provides us with suggestive and useful illustrated corpus of the principal productions belonging to the Gravettian period. One of the major conclusions to emerge from this material, as he demonstrates in a series of brilliant analyses of particular productions, is the astonishing variety of technical and artistic mastery that they show. It is difficult to think of a single preconception based on the earlier art studies of more elaborate societies which has any direct relevance to this art as we find it at the earliest stage of the Palaeolithic tradition.

Undoubtedly the most significant group of works of this period are the so-called Venuses already referred to. The character of these is sufficiently well known, namely, obese naked figurines of women sculptured in the round, to require no further description here. What Graziosi does make abundantly and most usefully clear is the nature of the stylistic variation between the different figures, and also of almost equal importance their geographical distribution. From a stylistic point of view we see that they range from highly abstract or geometricized forms such as those of Lespugues in the Pyrenees through startling pieces of realism such as the rough hewn but highly naturalistic modelling of the Savignano specimen in North Italy, and such masterpieces as the much smoother and more voluptuous lady from Willendorf in Austria. Recent work in Russia has shown the astonishing profusion of such statuettes in that area particularly north of the Black Sea and north-westwards to the borders of the Ukraine. Although here as elsewhere all the statuettes are not on the same

level of achievement and some are more successful than others, nevertheless all the principal elements of stylization and naturalism found further west occur, except perhaps the extremes of the particular kind of stylization shown at Lespugues.

Among the main general conclusions to emerge with certainty is the fact that contrary to many earlier assumptions three-dimensional modelling of the utmost mastery makes its appearance at a very early stage in artistic evolution. As such it is used both for an advanced form of abstraction and also for emphasized or straightforward and extremely competent and well-observed realism. It is indeed difficult to think of a single one of the preconceptions of the earlier art historians that has found support in the study of Stone Age art. In a word any conceptions of what is to be regarded as 'primitive' and what as 'sophisticated' would appear now to require rethinking from first principles.

The most interesting historical problem to be found here is no doubt whether the art as the archaeological record has preserved it, is in fact a sudden phenomenon springing like Athena from the head of Zeus, or whether in actual historical fact the stage as we know it was in reality preceded by a long unrecorded development of unknown character. Here it should be remarked that the new Russian evidence contains important potentialities. This is perhaps a point to which the author does not give quite adequate weight. For we now know that the stage associated with the Venuses, that is to say, of the fully evolved 'East Gravettian' in South Russia, was one which was preceded by many millennia of gradual cultural development of specifically Upper Palaeolithic character. To the west indeed the immediate predecessors of the Gravettians were the Aurignacians and we have seen that some slight traces of a primitive art tradition can be found among them also. But it is a new acquisition of archaeology that east of the Carpathians lay a huge and hitherto little understood province of cultural development in which there was a long history of social and cultural development preceding the particular stage of the Gravettian associated with the initial artistic productions in that area.

At the time of writing the details of these earlier culture stages, although extremely interesting, are tantalisingly limited in kind, since the fact that no artistic productions have yet been associated with them is far from allowing us as to assume that no such productions were made during this early period. Very early traces of archaic or ancestral Upper Palaeolithic have also been known for some considerable time from South-West Asia and have recently been discovered by the writer in North-East Africa. Here again no definite signs of artistic production have yet come to light, but once again the material available for study, that is to say, the material traces of the actual number of communities and episodes, is not sufficient for us to attach yet conclusive importance to the negative evidence, though it is stronger than in Russia.

The alternative approach to this problem of the nature of the earliest art is through psychological theory and observations. Graziosi has a short but an illuminating section on this point and discusses the well-known theories of Luquet which he ends by largely rejecting. It may be that he is perhaps a little unfair on an investigator who has after all the merit of being a pioneer in this field. Luquet's notion, apparently based on child art, suggested that there was an initial phase of 'fortuitous realism' when accidental resemblances to natural objects might be observed from time to time in the course of random scribblings made for their own sake. Once the accidental resemblances had been noted they might eventually lead to 'intentional realism' when resemblance was the primary objective. But the truth may still be that no serious analysis in the light of modern psychology has yet been attempted. If so here is another threshold to a most interesting and fruitful study of fundamental problems to be tackled in the future.

The question of the later development of Palaeolithic art, that is to say, the long-term

trends of stylistic development, and changes in content perceptible over the enormous period of its duration, is one which has not perhaps been generally understood as well as it might have been by those not immediately concerned with this type of art history. Graziosi summarizes with a wealth of illustration the principal pieces of evidence bearing on this subject and it cannot be said that his analysis is other than conservative. Even so he demonstrates beyond question the nature of the peculiar character of the early Gravettian (Perigordian) style. He shows moreover that it is no short-lived archaic phase, but on the contrary its principal characteristics survive at least as late as the 16th or 17th millennia B.C., that is to say, through the Solutrian and into the earlier stages of Magdalenian. This is a most important fact that probably has not received its due recognition as yet even among many prehistorians.

The cornerstone of our understanding of these earlier periods is indeed a relatively small group of dated finds, though they are remarkably consistent in their testimony regarding the state and characteristics in a broad sense of the art of the times. Probably the most important single document is that of the Perigordian engravings on the walls of the Pair-non-pair cave. It is perhaps a pity that Graziosi could not include the fine reproductions of these already published by Breuil in *400 Centuries of Cave Art*. Nevertheless he makes very clear the prevalence of definite trends of stylization or schematic distortions—for instance of the head and neck of horses, and the nature of the outline, in particular the treatment of the legs and profile. To the reviewer it would appear that he might have made a little more of certain other features such as the systematic distortion of the head and neck in relation to the trunk of various animals, and certain additional schematizations such as those affecting the snout of horses, and the general architecture of their heads. By his choice of figures and his description however he does undoubtedly bring out one extremely important point, namely, the astonishing vivacity and life in these early works.

In the comparison he draws between this early style and the later stage associated with the Middle and Late Magdalenian Graziosi has many interesting things to say. Perhaps most penetrating and original are his comments on the subject of composition. This, as he points out, has probably been underestimated in a number of cases. It is true that one of the overall characteristics of prehistoric art, more particularly of the parietal art, and in the decorations on the larger mobile objects, is the absence of the sense of frame or defined space. Except in the case of a few minor decorative devices this limitation in the matter of composition is almost universal in Palaeolithic art. There is however in the later phases, particularly in the elaborate forms of decoration applied to spear-throwers, arrow-straighteners, and the like, a sense of composition of a different kind which attains a remarkable level. This is especially noticeable in the adaptation of figures to their purpose such for instance as the famous leaping horse at Bruniquel, to the form of the spear thrower and the adaptation of figures following one another around the cylindrical handle of the Lorthet baton. Here is a realm of achievement in Palaeolithic art to which we must be grateful to Graziosi for drawing our attention so clearly.

But a very important question concerns the nature of the contrast between what we may call the early Gravettian tradition and that of the high Magdalenian. There are of course a number of features that leap to the eye, such as the generally greater mastery of the visual impression and its recording in particular figures. In the matter of detail the list of distinguishing features is indeed a long one, but more important than any one of these for a proper understanding of the nature of the development as a whole is undoubtedly the nature of the intention underlying the principles of representation. Graziosi indeed shows himself sensitively aware of the profound difference in quality and as it were in texture of

the two manifestations in question; but when it comes to propounding an explanation for these differences it seems to the reviewer that he has neglected one factor which was worth serious consideration. Subtle and all pervading though such stylistic differences appear to be from an aesthetic point of view, it may well be that in certain cases they derive from a single relatively simple and concrete change in attitudes on the part of the artist.

Graziosi does not seem to consider this possibility and this is the more unfortunate since an explanation along these lines can in fact be propounded on the strength of a theory by P. A. Leason offered in 1939 in the pages of the Prehistoric Society. It does certainly seem to the reviewer that Leason's theory has been most unjustly neglected since it offers at least a possible explanation and one of far-reaching importance. Many readers of ANTIQUITY will perhaps be familiar with it—in essence it consists simply in the suggestion that the bulk of the products of the Middle and Late Magdalenian period show not living animals such as the deer crossing the stream from Lorthet, but simply dead animals lying on the ground. To some minds, perhaps those of a more enthusiastic or journalistic character this idea seems to be unacceptable for no better reason than that it appears to belittle the achievement of the Stone Age art which they so much admire. The fact is however that Leason's study is based on most careful observations and propounded with much critical sense; although being a non-specialist in prehistoric studies, he makes the mistake of supposing that all Stone Age art shows the same stylistic characteristics and fails to understand the profound idiosyncrasy of so many of the earlier productions. The point has always struck me that Leason's theory applies to the Magdalenian and to the Magdalenian alone. Within this field his suggestion has very considerable force. Moreover, being an animal artist himself, Leason is naturally sensitive to certain features which the pure archaeologist easily misses and to this extent also his opinion must carry a certain authority. He notes the attitude of the head so often stretching away and upwards from the body, and above all the consistent drawing of the legs furthest from the observer as longer than those which are nearer to him. Other features cited are the stiffness of the limbs, and he was able to show that whereas these are a constant characteristic of dead animals, or animals that have only been dead for a short time, they are completely at variance with any visual impression created by a moving or a standing animal. It is true that he is wrong in supposing that every product of the Magdalenian period can be explained in this way. One of the reindeer on the slabs from Limeuil for instance is quite clearly shown standing, and it is a remarkable fact that it should be in true perspective with its feet standing squarely on the ground. It may well be also that many of the horse figures such as the famous Font de Gaume horse in chiaroscuro are intended to be in full and lively movement. None the less at least 75 per cent of the productions of the late Magdalenian period and among them many of the most famous, such as the bison in curious tiptoe attitude at Altamira, and many hundreds of others can be most reasonably explained in Leason's manner.

In contrast it is most remarkable that the works which are either certainly dated to the Perigordian such as the engravings at Pair-non-pair, or those which by comparison to the dated remains can with reasonable probability be attributed to a period antedating the Late Magdalenian, at Lascaux for instance, are wholely devoid of these peculiar characteristics. It is to be hoped that investigators of Professor Graziosi's standing may give this matter in the future the attention it deserves.

The question of the geographical distribution of particular stylistic features is one which is also basic to our understanding of the nature and meaning of Palaeolithic art. As Dr Graziosi's admirable maps show all the later stages of the art in general, and the engraving and painting of cave walls in particular, is concentrated in South-West Europe, that is to say, South-West France and Spain. The lack of such art in East Europe would seem to

require a special explanation, though Graziosi does not enter into this in detail. One may indeed wonder whether the explanation does not lie in the recent studies of Russian Upper Palaeolithic. Here it has been shown conclusively that no art except in the most rudimentary form survives to the later stages of the Upper Palaeolithic. There is indeed a specific hint that here development included a swing from interest in naturalistic and representational art to the elaboration of geometric art as best seen at the famous but relatively ill-dated site of Mezine, followed by swift disappearance. Hints of a similar development may be seen for instance in the Ahrensburgian and Hamburgian in North Germany. Thus it may be said with a certain confidence that the cave dwellers of Central and Eastern Europe contemporary with the Magdelenians and Solutrians of the west, were truly ignorant of the practice of decorating the rock walls. There is indeed a good deal to suggest that in the earlier period also the true area of initiation of this practice also lay in south west France. Progress in the study within south west Europe of the distribution of cave decoration shows moreover an interesting degradation from area to area. Thus the marginal areas of South Spain, the Rhône Basin and the recently discovered Italian province have many interesting features in common but rarely if ever achieved the aesthetic heights, and never the full range of expression, of the more outstanding products of the central area in South France and North Spain. Here again would seem to be a most interesting and promising field for future investigation.

As to the inner chronology of cave decorations it seems likely as Professor Graziosi points out that we can hardly hope for the same sort of precise chronological segregation which has been achieved in the field of the mobiliary art. Here stylistic comparisons with the occasional dated fragments and with mobiliary art itself are bound to be of a more or less subjective nature though supported to some extent by the study of superposition. More objective study of superpositions patiently complied in very large numbers should eventually perhaps lead to a rather more definite structure of comparison than Dr Graziosi allows. One might mention for instance such factors as those provided by photography with lights of different spectra and, in the case of engravings, the minute examination and production of microcasts of the intersection of scratches.

Although interesting and suggestive and based on the authority of an enormous experience in the reconstruction and minute examination of particular finds, yet the elaborate schemes of the Abbé Breuil in this field are hardly apt to provide anything in the nature of a definitive sequence. On this point the author supplies a remarkably sane and well considered analysis, which should prove a useful starting point for further studies. Although the simultaneous reproduction of photographs and copies does indeed lessen one's confidence in the details of some of the Abbé's reconstructions it is only fair to add in the case of the paintings that not a few have undoubtedly been seriously affected by weathering since their original discovery half a century ago.

Thus Dr Graziosi's work, although the regional contributions lie mainly in the aesthetic and subjective aspects of the study provides a most generous panorama, abounding in informative illustrations and nicely balanced in approach. It is to be hoped that the manifest and loving labour that he has put into it serves to stimulate others to cooperate in the laborious but much needed task of completing the iconography and undertaking the exact analysis from which alone a fundamental advance in this fascinating subject can result. It would be ungrateful to conclude without a word of appreciation for Messrs Faber and Faber in venturing to produce a work on such a magnificent scale. Let us hope the public for archaeology and art history will match the publishers enterprise with a worthy response to the opportunity they are offered. The reviewer will be surprised if copies are not collectors' pieces changing hands for half again the price in ten years time.

A New Bronze Sword from Cyprus[1]

by HECTOR CATLING

Mr H. W. Catling of the Ashmolean Museum, Oxford published in 1956 *in the* Proceedings of the Prehistoric Society *a critical catalogue of Bronze Type II swords in the east Mediterranean. He there paid particular attention to a weapon in the Cyprus Museum which had been newly noticed. Here he publishes another weapon of this class and discusses afresh the origins and inter-relationships of these swords.*

1. THE NEW SWORD

This new sword comes from the collection of Mr Evangelos Loizou of Famagusta, and has been brought to my attention by my former colleague Dr Vassos Karageorghis of the Cyprus Museum.[2] The sword (PLATE XVI (a)) was given to Mr Loizou's father by a villager of Enkomi some years ago, and almost certainly comes from the famous Late Bronze Age city and cemetery site a few hundred metres west of the village.[3] It could well have come from a grave. The sword lacks the extreme tip; one edge is chipped. Apart from superficial corrosion it is in excellent condition.

The preserved length is 0·47 m.; the handguard is 0.05 m. wide, and the length of the hilt, 0·10 m. The greatest width of blade (below handguard) is 0·035 m. The rivets diminish in length from pommel to handguard in the range 0·023–0·018 m. There are two rivets in the handgrip and six in the handguard. The pommel ears are prominent, but there is no spur. The hiltplates were made of some perishable material, but, as often, their outline can be restored from the 'shadow' left by the corrosion pattern on the handguard. The junction of blade and handguard is more angular than on any other Mediterranean example known to me; also unusual is the even taper of the blade from handguard to tip in place of edges normally either parallel, or even slightly swelling.[4]

The weapon is only just long enough to be classed as a sword,[5] and is well below average length (0·63 m., taken from fifteen complete or nearly complete examples, ranging from 0·82 m.–0·45 m.). The rivet arrangement occurs on one other sword, that in Swedish Tomb 18 at Enkomi.[6] This is a very erratic feature, however, and I recorded nine different combinations[7] in fifteen swords, of which four and four is much the commonest, occuring five times. There are pommel spurs in seven out of fourteen weapons whose hilts are fully preserved. Blood channels on the blade are a normal, but not unvarying feature, occasion-

[1] My earlier paper, 'Bronze cut-and-thrust swords in the East Mediterranean', was published in *PPS*, XXII, 1956, 102. During the preparation of this present paper I have received valuable help from Miss N. K. Sandars and Mr J. D. Cowen, particularly with regard to details of swords previously unknown to me. My wife has drawn the map, FIG. 1.

[2] My thanks are also due to Dr Karageorghis for the photographs, PLATE XVII (a) and (b). Dr Karageorghis has published a brief note on the sword in *BCH* LXXXIV, 277 ff.

[3] There are many accounts of this famous site; see especially Murray, Smith and Walters, *Excavations in Cyprus* (1900), 1 ff. and C. F. A. Schaeffer, *Enkomi-Alasia*.

[4] cf. *PPS*, XXII, pl. 9. *Enkomi-Alasia*, fig. 105.

[5] cf. G. H. Gordon, 'Swords, Rapiers and Horse Riders', ANTIQUITY, XXVII, 67 ff.

[6] *PPS*, XXII, 115, no. 16.

[7] *op. cit.*, 109f.

ally being replaced by ridges in groups of one or more on either side of the medial line, particularly on the later weapons.[8]

2. CLASSIFICATION OF TYPE II SWORDS (FIG. 2)

Mrs Vermeule[9] has recently commented on the degree to which the swords differ from one another in details of this kind, and suggests that such differences must imply a number of smiths producing the weapons at a number of different sites. I hope to show below that

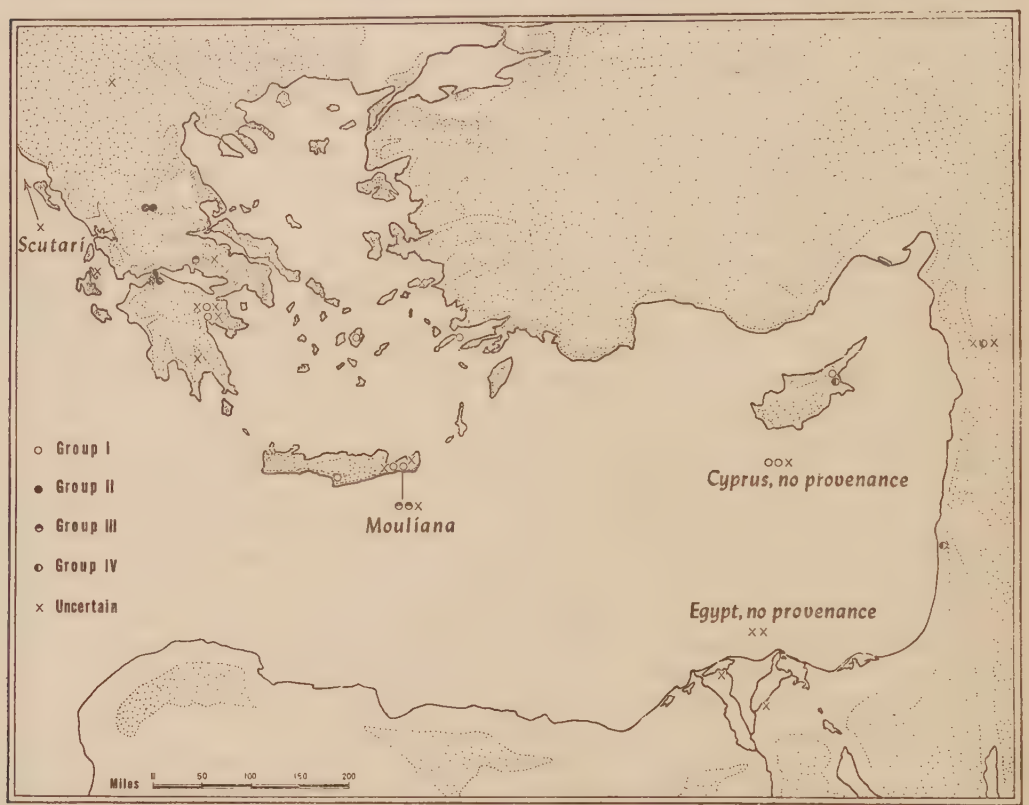

Fig. 1. Distribution of Type II swords in the east Mediterranean.

these differences may be reduced to a typological coherence of considerable interest. Some of the points made in my paper in the *Proceedings of the Prehistoric Society* stand in need of correction. I take the present opportunity to revise some of my conclusions and to suggest a new classification of the Type II swords in the east Mediterranean, illustrated by the Distribution Map, FIG 1. For convenience, I include a very summary list of the swords, together with details of several either inadvertently omitted from my *PPS* catalogue, or found since. I refer to the forty-one swords now known to me by serial number and site. Numbers in brackets are those in the *PPS* catalogue (wherever they are different), to which reference should be made for the description and documentation of individual pieces. The list is arranged by region.

[8] e.g. *op. cit.* 113, no. 10 from Orchomenos.
[9] *AJA*, 64, 14

GREECE: 1, 2 and 3: Mycenae. 4 and 5: Tiryns. 6, 7 and 8: Achaea.

9: Laconia, Palaeopirgos.[10] Displayed in the Sparta Museum, apparently from a tomb excavated in 1957. No other details available to me.

10 (9): Phokis. 11 (10): Boeotia, Orchomenos.

12: ?Thessaly, Graditsa. (PLATE XVI (a) and XVII) Oxford, 1927:1383. Length 0·846 m. Length of hilt, 0·138 m. Width of handguard, 0·051 m. Width of blade, 0·03 m. Length of pommel spur, 0·042 m. Rivets, 0·023 m. In the handguard were 3 rivets, 2 on one side, 1 on the other. There were originally 4 in the handgrip; 3 survive. There are pommel ears and prominent spur. There is a parallel ridge on either side of the medial line. The edges of the blade are parallel almost throughout their length. The handgrip swells slightly.

13: The same. (PLATE XVI) Oxford, 1927:1384. Length, 0·866 m. Length of hilt, 0·145 m. Width of handguard, 0·057 m. Width of blade, 0·035 m. Length of pommel spur, 0·044 m. All rivets are lost; there were originally 4 in the handguard and 5 in the grip. Otherwise very close to 12.

These two swords (nos. 12 and 13)[11] formed part of the Sir John Evans Collection, given to the Ashmolean by his son. The only clue to their origin is a paper label on each one 'Graditza, 1891' in Sir John's handwriting. Wace and Thompson made no reference to them in *Prehistoric Thessaly*.

CRETE: 14, 15, 16 and 17 (11, 12, 13 and 14): Mouliana, Tombs A and B.

18: Karphi.[12] A fragment, from between hilt and point. Length, 0·223 m. Occupation of the settlement covers the transitional period 1100–900 B.C.

19: Siteia.[13] Now Cambridge, Fitzwilliam Museum GR. 94a. 1906. Formerly Dawkins Collection. Hilt incomplete. Preserved length 0·56 m. The pommel ears are broken away; it is not clear whether there was a spur; 2 rivet holes low down on the handguard, but none in the grip. Two relief ridges on either side of the medial line. Undated.

20: Vrokastro.[14] A dirk, tip and pommel lost. Preserved length 0·215 m. 2 rivets in the handguard, 1 in the grip. Swelling handgrip. The context is unknown to me.

21: Myrsine, Aspropilia (district of Siteia).[15] Length 0·50 m. 2 rivets in the handguard, 3 in the handgrip. There are pommel ears, but no spur. Blood channels. Found in a recently excavated tomb. With the burial to which this sword belonged was a LM III B–C stirrup jar. The other contents of the tomb were of LM III B date.

22: Plain of Mesara.[16] Herakleion, Giamalakis Collection. Measurements are not available to me. It has pommel ears and spur. There are 4 rivets in the handguard, 3 in the grip. There are blood channels. No context.

ISLANDS: 23 (29): Ithaka, Polis.

24: Samos.[17] The hilt and upper part of the blade are lost. Existing length 0·38 m. Two parallel ridges on either side of the blade. Found in the German excavations, apparently in a Geometric context.

25: Naxos, Grotta.[18] Length 0·65 m. Greatest width of blade, 0·04 m. There are 4 rivets in the handguard, 4 in the grip. Pommel ears, but no spur. Found in a chamber tomb, together with Myc. III C 1 pottery.

26 (29): Kos.

[10] I owe this information to Mr. A. N. Snodgrass.

[11] First brought to my attention by my colleague, Mr. H. J. Case.

[12] *BSA*, 38, 117 and pl. XXIX.2, no. 500.

[13] Mr R. V. Nicholls kindly provided details of the form and dimensions of this sword.

[14] Montelius, *La Grèce Préclassique*, pl. 14, no. 8

[15] Dr N. Platon, Ephor of Antiquities in Crete, has most kindly allowed me to mention this sword, and has supplied details.

[16] *Jahrbuch d.RGZ Mus. Mainz*, 2 (1955), 159, Abb. 3. 21.

[17] Miss Sandars has given me details of this find.

[18] I am indebted to Mr V. R. Desborough for my first knowledge of this sword, and of 21. I am most grateful to the discoverer, Professor N. Kondoleon for providing details and allowing me to mention the weapon.

CYPRUS.[19] 27[20], 28, 29, (16, 17, 18) 30 (The Loizou sword), 31 (19).

PERIPHERAL. 32 (27): Albania, Scutari.

33: Macedonia, Monastir.[21] Length 0·68 m. Handguard width 0·047 m. Greatest width of blade, 0·036 m. There are 2 rivets in the handguard, 2 in the grip, the latter of which seems not to be complete. Neither ears nor pommel spur survive. There are four parallel ridges on either side of the elliptical midrib. The handguard profile is angular. It was found at Monastir in 1899 during an excavation of *l'Institut Russe*. The context is unknown to me.

34,[22] 35, 36 and 37 (20, 21, 22 and 23): Egypt.

38: Palestine, Megiddo.[23] A dirk, length 0·317 m. There are 2 rivets in the handguard, 1 in the grip. Small pommel ears, long thin spur. From the Iron Age levels.

39, 40[24] and 41 (24, 25 and 26): Syria, Hama.

Comparatively few of these swords are from well dated contexts, but what evidence there is suggests that most fit into the period 1225–1125 B.C., though some of the typologically more advanced are of post-Bronze Age date.

3. THE ORIGIN OF TYPE II SWORDS

The origin of Type II swords in the Mediterranean is controversial. The older authorities attributed them to Central Europe, but this was questioned by Childe in a famous paper[25] in which he sought them in the Near East, seeing prototypes in a sword at Alaça Hüyük and the four weapons (surely castings) at Ras Shamra. I showed[26] that a derivation from the Near East could not stand. I suggested instead that, despite the obvious divergencies between Type II and the Aegean sword series proper, a case could be made out for Type II starting in the Aegean under the special type of stimulus which would have been created by the turbulent conditions which accompanied the end of the Aegean Bronze Age. This view was mistaken.

J. D. Cowen, who has made an exhaustive study of the European swords[27], has shown that earlier versions of Type II swords, namely Sprockhoff's Types Ia and Ib, are unknown in the Mediterranean. Sprockhoff IIa (= Type II) is common to both the Mediterranean and to Central and North Europe. The existence in Europe of a parent form unknown in the Mediterranean must, I think, be taken as conclusive evidence that the old view of the European origin of Type II swords in the Aegean and the Levant was right.

I now see that the East Mediterranean swords can be arranged in a number of groups which in many respects correspond to European groups defined by Cowen. The main criterion used is the shape of the hilt, particularly of the pommel. Subsidiary indications are the placing of rivets, and the appearance of blood channels or ridges. My groupings are as follows. (cf. FIG. 2)

[19] The fragment figured by J. Naue—*Die vorrömischer Schwerter*, Taf. VI:6 and p. 13—should be discounted. I also ignore the fragment figured in *The Bronze Age and the Celtic World*, pl. 12.11.

[20] The length of 0·75 m. for this sword given in Gjerstad *et al.*, *Swedish Cyprus Expedition*, I, 553 no. 70 seems to be a misprint for 0·65 m.

[21] Miss Sandars has generously made available to me notes taken on this weapon in Istanbul.

[22] This is the famous sword with the cartouche of Seti II; considerable doubt has been thrown on its identification as a Type II sword, particularly by V. Milojčić, *Germania* 30, 95 ff.

[23] *Tell-el-Mutsellim*, II, 45 f. and fig. 45.

[24] I mistakenly said *PPS*, XXII, 117—that none of the Hama swords is illustrated; this sword is figured *Hama*, 120, fig. 136B.

[25] 'The Final Bronze Age in the Near East and in Temperate Europe,' *PPS*, XIV, 177 ff.

[26] *PPS*, XXII, 120 ff.

[27] *Bericht d. Röm. Germ. Komm.*, 1955, 52 ff.

GROUP I. Fish-Tail Hilts. (Corresponds to Cowen's Nenzingen Group.[28])

List nos. 1, 15, 21, 25, 26, 27, 29 and 30. The pronounced wide V-outline of the pommel ears, uninterrupted by a spur, is diagnostic. Rivets vary between 8 and 5, with much difference in arrangement. The size is in the range 0·46 m. to 0·65 m., with an average of *c.* 0·56 m. (contrasting with the European average of 0·61 m. quoted by Cowen.)[29] Blood channels are a constant feature.

21, 25, 26 and 27 come from groups which probably cover the period 1225–1175 B.C.; 15's date is *c.* 1200–1125 B.C.

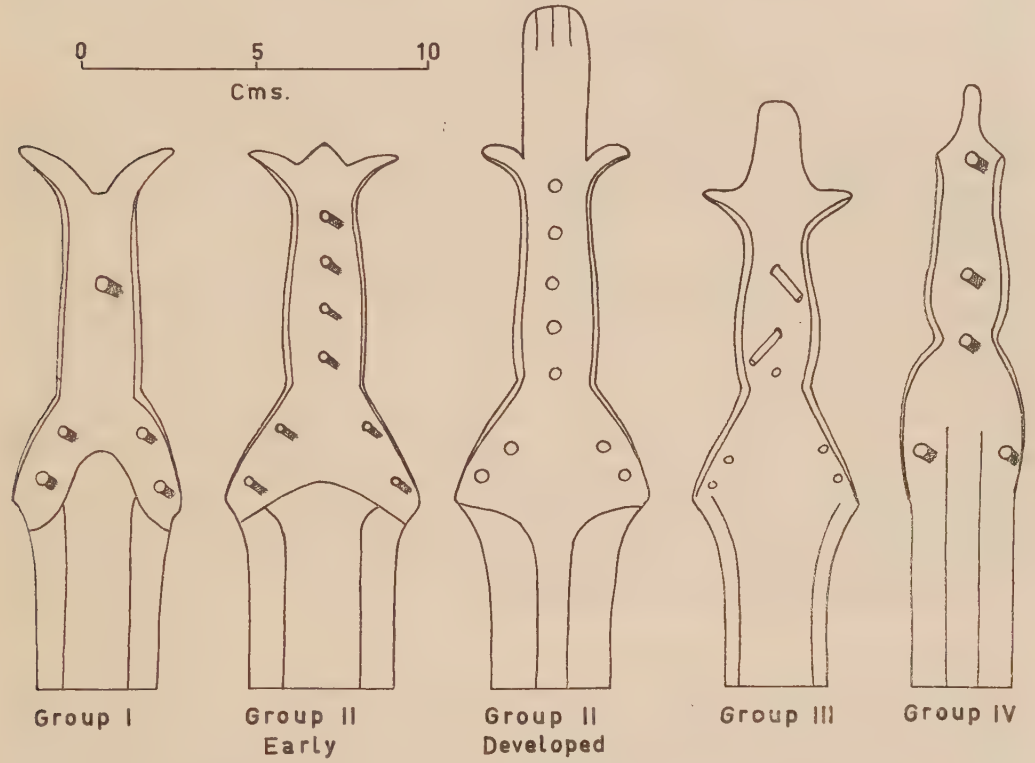

Fig. 2. Diagram to show development of Mediterranean Type II swords.

GROUP II. Spurred hilts, large swords (corresponds to Cowen's Erbenheim Group).[30]

List nos. ?5, 6, 7, 8, 12 and 13. Initially the hilts closely resemble those of Group I, with the addition of a spur in the middle of the fish-tail. This starts as a low triangular protuberance (as on 8), but quickly increases, so that on some (e.g. 13) it is half as long as the handgrip. The ears are flatter than Group I, though still curving, except 12, which has straight topped ears, looking forward to Group III. Erbenheim swords seem only to have ears of 12's type. The introduction of the spur and the flattening of the ears suggests a significant difference between the pommels of Groups I and II. Group I must have had a cup-shaped hollow at the top to account for the fish-tail. Evidently in Group II this hollow is eliminated, and the pommel top flattened with a

[28] *op. cit.*, 63 ff.
[29] *op. cit.*, 64.
[30] *op. cit.*, 73 ff.

knob protruding at the centre. On 12 and 13,[31] the tip of the spur protruded above the top of the pommel knob. Group II swords are substantially larger than Group I. Three, perhaps four (7, 12, 13 and ?5) are over 0·80 m. The average length of 0·775 m. compares with the 0·69 m. of the Erbenheim swords,[32] the largest of which is only 0·745 m., by comparison with 0·86 m. All (discounting 5) have ridges in place of Group I's blood channels. The rivets vary between 10 and 7; 5 has none, and is possibly an unfinished casting. Erbenheim swords are equally prolific in rivets.

6, 7 and 8 are from excavated groups associated with Myc. III B–C pottery.

GROUP III. Spurred hilts, smaller swords (corresponds to Cowen's Lettener Group).[33]

List nos. 10, 14, 16 and 22. Basically, a flimsier version of Group II, but always with straight topped ears; 22, at least, shows the slightly 'leaf-shaped' blade which is characteristic of Lettener weapons. Blood channels replace the ridges of Group II. Rivets vary between 7 and 5.

The context of 14 is *c.* 1200–1125 B.C.

GROUP IV. Small swords, vestigial ears (unrepresented amongst Cowen's groups).

List nos. 28, 38 and 40. These derive from Group III, with which they share the pommel spur and small dimensions. The width of the spur tends to become narrower in relation to its length, while ears diminish in size—e.g. 38—and eventually disappear—e.g. 40. They are all under 0·50 m. in length, while rivets vary between 5 and 2. They are inconsistent in the details of their blade section.

28 may be as late as the mid-12th century; 38 and 40 are of Iron Age date.

I have so far confined myself to swords with hilts complete or nearly so; some of the damaged or fragmentary pieces can be attributed conjecturally. Group I: 23, 31 and 37. Group III: 11 and 17. Group IV: 20, 39 and 41. The remainder I cannot attribute with any confidence; 19, 24 and 33 would not fall precisely into any group since they have two or more ridges on either side of the medial line.

Group I occurs at Mycenae, in Crete, Naxos, Kos and Cyprus. Group II provides all the Achaean swords, those from ?Thessaly, and perhaps one from Tiryns; Group III appears in Phokis and Crete and Group IV only certainly in Cyprus and the Levant.

Note has already been taken of the correspondence of Groups I–III with Cowen's Nenzingen, Erbenheim and Lettener groups. In Europe, Nenzingen swords appear during Reinecke's Bronze Age D,[34] whereas Erbenheim and Lettener seem not to occur before Reinecke's Hallstatt A.[35] This evidence is in keeping with the Aegean finds, where the dated examples of Group I are likely to be somewhat earlier than Groups II and III.

4. DISCUSSION

Is it possible to decide whether all three groups that have obvious European parallels come to the Aegean from Europe? It must be conceded at once that Group I is of European origin. Where, however, does Group II get its spur? Study of the swords convinces me that the spur is an alien feature added in an environment where swords with a different type of pommel were current. It might have been the Aegean, where dome-shaped pommels[36] had been used for generations. If so, the Type II sword must have returned to Europe with Aegean modifications. Perhaps Group II is an Aegean version of the Nenzingen sword, adding the spur and replacing the blood channels by ridges (Aegean armourers were past masters at embellishing weapons in relief). Some Group II swords must return to Europe, and Group III will then represent a second northern influx introducing a northern modification of Group II, with the size reduced, the blood channels

[31] I owe this observation to Mr. M. R. Popham.
[32] Cowen, *op. cit.*, 73.
[33] *op. cit.*, 78 f.
[34] *op. cit.*, 64 ff.
[35] *op. cit.*, 73 and 79.
[36] cf. the sketches of types published by S. I. Dakaris, *Eph. Arch.*, 1956 (1959), 140, fig. 9.

restored, and the blade profile developed to the characteristic leaf-shape. It is noticeable that Erbenheim swords (the European version of Group II) tend to have an incipient leaf-profile, whereas Group II is straight sided. This hypothesis fits the difference in date in Europe between Nenzingen swords and Erbenheim and Lettener, giving them time to be modified in the Aegean, return to Europe and be remodified. Group IV clearly developed out of Group III in the Mediterranean without any interference from the north, or any cross-influence.

Under what circumstances did the swords first come to the Aegean? There are three main possibilities. (*a*) They were traded. (*b*) They were brought by barbarian invaders. (*c*) They were brought by barbarian mercenaries, enlisted by hard-pressed Mycenaean princes at a time of great disturbance. (*a*) is weak, since such trade had been in progress since at least L.H. I (amber, etc.) without any corresponding introduction of northern weapons. The general archaeological record does not suggest wholesale barbarian invasions at the end of the 13th century, and I therefore propose (*c*) as the most likely explanation. There are many instances of civilized societies having to rely on the military prowess of their less advanced neighbours at such periods of disintegration. Moreover, the old trading associations between the Aegean and the north would have provided the necessary link.

The distribution suggests that these mercenaries may have travelled overland south to the Adriatic, and thence by sea up the Gulf of Corinth—they may even have been shipped in Mycenaean hulls. It is tempting to conjecture that they were brought in by a confederacy of the princes of the Argolid and Achaea controlling between them the sea lanes westward towards the Adriatic and eastwards through the islands to the Levant and Egypt. It certainly looks as though the further distribution south and east (Crete and the Levant) took place from the Argolid.

It is noteworthy that three, perhaps four of the Cypriot Type II swords are of Group I. No. 27, from Enkomi Tomb 18, shows that the weapon had reached Cyprus at the very beginning of the influx of Aegean settlers which was to transform the history of Enkomi in the 12th century. The Enkomi Group IV sword, No. 28, suggests a second influx of Aegean settlers, well into the 12th century, which harmonizes with the ceramic evidence from the site.

Taking the distribution, and ignoring the Iron Age examples from Hama and Megiddo, one can see that nearly all, apart from the Egyptian ones, come from areas under direct Aegean control at the time they were current. It is customary, moreover, to explain the Egyptian swords[37] by reference to the Akaiwasha who served as allies or mercenaries of King Mer'eye of Libya during his attack on the Delta at the end of the 13th century, equating them with the Achaeans. If so, the direct link with the Aegean is preserved here too.

I have conjectured barbarian mercenaries from central Europe as the bearers of the new weapon into the Aegean. It is improbable that they were the only users once the type became familiar. Indeed, Type II swords seem largely to have ousted Aegean types. If the relatively common cast-hilted dirks[38] are set on one side, the only Aegean swords contemporary with Type II seem to be either the rare type with cast-hilt and widening blade found in Crete[39] and Cephallenia[40], or the almost equally rare cast-hilted weapons with

[37] H. L. Lorimer, *Homer and the Monuments* (1950), 87 and references.
[38] cf. e.g. Furumark, *Chronology of Mycenaean Potery* (1941), 94, fig. 4, b1 and b3.
[39] Lorimer, *op. cit.*, pl. XIX:3.
[40] *Eph. Arch.*, 1932, pl. 16, left.

cruciform, but down-turned handguards.[41] The former type appears to be a fusion of the classic Minoan/Mycenaean sword with the cast-hilted dirks, not with Type II swords, though it should be remembered that the Mouliana A swords have blood channels, nowhere else found in the Aegean series. This scarcity of other swords and the subsequent continuation of Type II in iron both suggest that the type was generally adopted.

5. A POSTSCRIPT ON GREAVES

I suggested some time ago that the bronze greaves found both in Greece and Cyprus[42] at the turn of the 13th–12th centuries had been developed in response to the changed style of fighting that one supposes ensued on the introduction of Type II, with which crippling blows at unprotected legs would have been so much easier than with the old thrusting swords. I felt encouraged in this belief by the appearance of a pair of greaves in the same tomb at Enkomi as a Type II sword and a similar close association in Achaea. Recently, G. von Merhart has urged the European[43] origin of such greaves, and an attractive picture emerged of barbarian mercenaries arriving not only with their distinctive swords but also a new type of armour. But bronze greaves were found in 1960 in a late 15th-century grave at Dendra in the Argolid[44] to show that such armour was used in Greece some 200 years earlier than the first Type II swords can have arrived, and both aspects of the theory have to be rejected—greaves are now most unlikely to be of European origin, and their first appearance in Greece is quite unconnected with Type II swords and swordsmanship. But we can see how they got into Europe.

[41] e.g. Furumark, *loc. cit.*, no. c2, and a new one from Perati, *Praktika* 1954, 96, fig. 5.
[42] *Opusc. Athen.*, II, 35 f.
[43] *Bericht d. Röm. Germ. Komm.*, 1956, 91 ff.
[44] Dr Paul Åström, Director of the Swedish Archaeological Institute in Athens allows me to make this reference to his recent excavation in collaboration with Mr N. Verdelis of the Greek Archaeological Service.

Book Chronicle—*continued from p. 102.*

SEASONAL FEASTS AND FESTIVALS. *By* E. O. James. *pp.* 336, *London, Thames and Hudson*, 1961. 35*s.*

THE APPLICATION OF QUANTITATIVE METHODS IN ARCHAEOLOGY. *Edited by* R. F. Heizer *and* S. H. Cook. *pp.* 358. *Chicago. Published for the Wenner Gren Foundation by Quadrangle Books* ($7.50); *London, Tavistock Publications.* 50*s.*

A STUDY OF HISTORY: VOLUME XII: RECONSIDERATIONS. *By* Arnold Toynbee. *pp.* 740. *London, Oxford University Press*, 1961. A reconsideration of the first ten volumes. We are told in the publishers blurb that 'archaeologists have been continuing to make new discoveries' and that 'in the present volume Dr Toynbee has taken as full account of the critics' and the archaeologists' work as he has found possible'. Our immediate judgement is that it ought to have been possible to take fuller account. But Dr Toynbee does not really like archaeologists; he says 'the archaeologists' temper is apt to be as ruthless as their progress is swift . . . archaeologists are not only apt to be ruthless: they are also apt to be temperamental and to allow themselves to be unduly swayed by passing fashions'. *Price:* 45*s.*

ST. NYNIA. *By* John MacQueen. *pp.* 105 *with endpaper map. Edinburgh, Oliver and Boyd*, 1961. 10*s.* 6*d.* A re-examination of some of the important points of literary and onomastic evidence for the career of St. Ninian.

[*continued on p.* 134.

ANTIQUITY XXXV, 1961

The Ancient Forest of Blackamore

by G. W. DIMBLEBY

Dr Geoffrey Dimbleby is a University Demonstrator in Forestry at Oxford University. He has made extensive studies of the moorlands and heathlands of Britain to find out what effect man has had in producing the vegetation and soil conditions we see today. This article gives a typical example of the results he is obtaining.

THE wildness and desolation of moorland, even when seen on a fine summer's day, can give an overpowering impression of land untamed by man, land in which man is the interloper and not the master. The word primaeval usually comes to mind and, indeed, even modern geography books sometimes describe these areas as the last strongholds of wild nature. It is recognized, of course, that man burns the moor at intervals to facilitate turf-cutting or to provide better conditions for his animals whether sheep or grouse, but as this practice has been going on from time immemorial and the moorland still looks the same, it is tacitly assumed that it has always been the same.

How far from the truth this assumption can be has recently been shown by an investigation into some archaeological sites on the watershed of the Cleveland Hills. Such sites contain organic remains, particularly pollen and charcoal, which reflect the contemporary ecological conditions. The conclusions reached here do not necessarily apply to all moorland areas, of course; the Cleveland Hills receive less rain than moorlands further west, so their early history may have been different. Today this relative dryness is reflected in the predominance of heather (*Calluna*) rather than the plants of blanket bog. Nevertheless, these North Yorkshire Moors today have as strong an atmosphere of wilderness as anywhere in the North of England. To anyone who knows them the old name of Blackamore graphically expresses their present character.

It is all the more surprising, therefore, to find these barren uplands liberally sprinkled with round barrows; one or two can be seen on nearly every skyline. Elgee[1] has shown that the region is in fact unusually rich in prehistoric remains of various kinds. Prehistoric man could apparently obtain a livelihood on these hills—though he was probably nomadic—whereas today, in spite of all that is known about land use, they are virtually waste.

Elgee[2] believed that the moorland vegetation was entirely natural to the area, having been derived directly from a pre-glacial flora which was able to persist because the Cleveland Hills were a nunatak during the glacial maxima—an unglaciated island in a sea of ice. Even during Elgee's life, however, this view was becoming untenable, and particularly when it was demonstrated by Erdtman[3] that a peat bog at Goathland revealed essentially the same post-glacial forest history as the rest of the country.

Through the kindness of Mr Raymond Hayes, who has an encyclopaedic knowledge of these hills, I was able to study sites of Mesolithic and of Bronze Age date, within two miles of one another, sites very different in themselves, but presenting a consistent picture of ecological conditions. The occurrence of Mesolithic microliths on the Cleveland Hills has been known for a long time. They occur characteristically on the higher hills where the

peat is eroding away, although little attention has been paid to their exact stratification in the soil. However, Mr Hayes took me to a site at an altitude of 1250 ft. O.D. on the main watershed between the heads of Farndale and Westerdale. This was near White Gill, on Stony Rigg. By cutting back an eroding peat face it was possible to find the flints *in situ*. They were lying flat on the surface of the bleached mineral soil, underneath a layer of black organic matter about 5 in. thick, and they were associated with a considerable amount of charcoal. This was examined microscopically and found to be oak (*Quercus*), alder (*Alnus*), birch (*Betula*) and hazel (*Corylus*). Above this black humus layer was a layer of brown peaty organic matter about 18 in. thick, apparently derived from the heather.

The site was systematically sampled for pollen analysis and is reported in detail elsewhere,[4] but for the present purpose reference only need be made to two layers: the surface of the mineral soil on which the flints were resting and the black humus horizon (0–5 in.) above the flints. In Table I the relative abundance of the woody species is shown, together with the ratio of non-tree pollen to tree pollen (NTP/TP%). If the latter value is less than 100 it can be taken that the vegetation was dense forest; if above 150–200 the country was open.[5] On this basis, the pollen of the soil on which the flints were lying shows that dense forest prevailed with alder, oak and hazel as the chief woody species. In the black humus above the flints the NTP/TP percentage had risen to 104, still indicating forest but of a somewhat more open type.

These facts show two important points. First, they indicate beyond all reasonable doubt that the Cleveland Hills watershed was forested in Mesolithic times and that the moorland vegetation as we see it today had no counterpart at that time. Secondly, from a comparison of the analyses above and below the flint layer it is apparent that the forest was becoming more open. The presence of abundant charcoal showed that fire occurred and the flints suggest that man was the cause of the fires. In other words, Mesolithic man was bringing about some modification of his environment—a conclusion at variance with the commonly accepted view that Mesolithic man did not disturb his environment because he did not fell trees and cultivate. Fire is, however, an equally effective means of destroying a forest.

About two miles westwards along the watershed is a group of round barrows known as the Burton Howes. They are on one of the highest parts of the moors, at an altitude of 1419 ft. O.D. Mr Hayes kindly brought to my notice that two of these barrows were being excavated, which gave me the opportunity of carrying out pollen analyses on the buried soils and the materials of which the mounds were constructed. The results of these investigations have also been reported in detail elsewhere,[4] but taken together with the evidence from White Gill an interesting picture of change in one small locality can be drawn.

It was shown by the pollen analyses that the surface of the buried soils of both barrows had been removed before the mounds were built. A layer possibly only 2 or 3 in. thick had gone, but it, of course, contained the contemporary pollen. However, each mound was built up of turves and on the assumption that these were cut locally and were contemporary with the barrow, it was possible to obtain comparative data. Moreover, one of the mounds (4D) had been increased in size after the insertion of a secondary cremation; at the interface between the primary turf mound and the secondary extension was a thin line of humus which probably represented soil development before the extension was made. Technically it was not good material to sample, but as the associated mineral soil was poor in pollen and would therefore give little contamination, the analysis may be used for our purpose here. Barrow 1A had not been extended in this way, so the turves were the only source of data from it.

The significant features of the analyses are presented in Table I. Assuming that the results are comparable, which is reasonable in view of the close proximity of the barrows

TABLE I

	ΣTP	Percentage of Tree Pollen								NTP/TP %	Percentage of Non-tree Pollen		
		Alnus	*Betula*	*Fraxinus*	*Pinus*	*Quercus*	*Tilia*	*Ulmus*	*Corylus*		*Calluna*	*Gramineae*	*Plantago*
White Gill													
Below flints . .	189	52	10	—	1	34	1	3	35	39	10	51	—
Above flints . .	515	44	15	1	1	34	2	3	62	104	17	56	1
Burton Howes													
4D turves . .	331	56	9	3	2	26	1	2	87	119	62	28	2
1A turves . .	132	57	9	5	1	27	1	2	101	168	53	37	1
4D primary surface .	66	55	5	2	3	36	+	—	106	223	69	25	1
Modern Turf . .	61	30	25	11	5	21	—	8	67	787	76	13	1

to each other, it would be deduced that when Barrow 4D was built the land was still largely forest (NTP/TP = 119%), though the barrow may have been set in a clearing. By the time Barrow 1A was constructed the clearing had been enlarged so that the forest had in effect receded (NTP/TP = 165%). Finally, the primary surface of Barrow 4D was covered over when the NTP/TP percentage had reached 223, so that the environs were by then largely deforested. For comparison, an analysis from the present surface of the moor is included which shows a very much higher NTP/TP ratio, a reflection of the continued deforestation not only of the uplands but of the adjacent valleys as well.

This general picture of the changes in the landscape of the Cleveland Hills can be filled in in more detail from the pollen analyses. Changes can be seen in the relative proportions of the tree species. Comparing the two pollen spectra from White Gill it is apparent that as the result of disturbance the amount of birch has increased at the expense of alder. This is to be expected as an effect of fire upon a forest of this type. Comparing these figures with those from the present moorland surface the same trend in the tree pollen is seen—birch increasing markedly whilst alder decreases, and oak remaining more or less constant.

It is perhaps surprising, therefore, that the tree pollen ratios from the Bronze Age barrows do not seem to be intermediate between the Mesolithic and the modern data. In fact they are remarkably similar to those from the forest at White Gill before the Mesolithic disturbance. This fact must be seen in relation to the time lag between the Mesolithic occupation and the Bronze Age.

There are two possibilities. One is that the Mesolithic and Bronze Age occupations were roughly co-eval. This may not be as ridiculous as it sounds in an area of this type, an upland fastness where a primitive people could live out of contact with later cultures. The second possibility is that there was a considerable gap in time and that such modification of the forest as had been caused by Mesolithic man was obliterated by re-growth and succession back to the climax condition. This would imply a cessation of activity by Mesolithic man followed by a long period of rest. The long period of rest may indeed have occurred, as the Neolithic people, who left abundant remains in other parts of North-east Yorkshire, apparently did not settle on the acid soils which now carry moorland; scattered flint implements and stone axes have been found, perhaps indicating hunting or trading activities over the hills, but no pottery has come to light. Bronze Age men would, therefore, have been the first people to follow the Mesolithic.

The ecological evidence cannot answer this question. The most obvious approach would be C.14 measurements on charcoal from both sites. There is evidence, however, that the impact of the two cultures on the forest was different. The floristic composition is practically unchanged during the period when the forest was being driven back by the Bronze

Age occupation; it does not show the increase of birch and decrease of alder which followed the Mesolithic occupation. This may mean that fire was not the primary agency of deforestation used by Bronze Age man in making his clearings. The pollen analyses cannot tell us what method was used, but by Bronze Age times felling would have become a feasible practice and we shall see later that grazing was probably important. Either alone, or both together, could have produced the observed effect.

Before leaving the forest vegetation and turning to the non-tree vegetation, there is one species which is of outstanding importance as an intermediate between the two phases; that is hazel. Table II shows how the proportions of hazel have fluctuated in two ways. Firstly, the column $\frac{C}{T+C}$ gives a measure of the change in proportion of hazel in the

TABLE II

PROPORTIONS OF HAZEL

	$\frac{C}{T+C}\%$	$\frac{C}{P+S}\%$
White Gill		
Below flints	26	17
Above flints	38	22
Burton Howes		
4D Turves	46	27
1A Turves	50	26
4D Primary surface	51	19
Modern Turf	39	7

C = Hazel (*Corylus*)
T = Tree pollen
P = Total pollen
S = Fern spores

cover of woody species. This rose steadily as a result of human influence, though in modern spectra the values are lower again. As a proportion of the total pollen rain, however, the pattern is slightly different. Hazel increased up to a point, but as conditions became open its relative importance gradually fell again, until today it forms an insignificant part of the pollen rain on the moors. These changes are easily explained ecologically. Hazel may occur as an understorey species in dense forest but in such conditions it does not flower abundantly. If, however, the tree canopy is opened up, the hazel will flower more vigorously giving the condition seen in the later of the two White Gill analyses. Once clearings were established the hazel seems to have become locally dominant, and as the woody species were driven back hazel probably formed a marginal zone around the edge of the receding forest. Hence we see an increase to a maximum followed by a decrease in its overall representation as its place was taken by non-woody species. Nevertheless, being the most abundant species on the fringe of the forest, it figures high among the pollen rain reaching the site from the forest. Today, of course, the forest pollen rain comes from the valleys, where the ecological pattern suggested above has been completely disorganized by a host of other influences. This great predominance of hazel pollen in spectra from prehistoric ages is widespread on acid soils (and possibly other types too) all over the country, from Mesolithic to the Iron Age. It is therefore to be seen not as a climatically induced condition, but as a direct result of man's impact on the forest. The present state of advanced dis-

integration of the native forest in Britain gives no clue to this past dominance of hazel; indeed the idea of hazel having been able to grow on the moorland soils we see today seems far-fetched. However, it has been shown elsewhere[4] that severe soil deterioration rapidly followed the forest clearance on such sites, so that the present-day soils cannot be compared with those existing during the forest and hazel phases.

Changes can also be seen in the non-tree vegetation (this cumbersome term is used because some plants of the field layer are not herbs but low shrubs, as, for example, heather). In the first analysis from White Gill there was little non-tree pollen and that was predominantly of grasses. After the forest had been opened up there was a higher proportion of non-tree pollen, a reflection of the greater amount of light reaching the forest floor. For the grasses this may not necessarily reflect a greater abundance, but could be due merely to increased flowering. The larger amount of heather pollen, however, probably represents an increase in the abundance of this species. It is a light-demanding plant which would respond to an opening of the canopy both by more vigorous flowering and by spreading.

This feature of heather becomes more significant when we turn to the Bronze Age analyses. Though the turves of Barrow 4D show that conditions were not far removed from forest, yet it is apparent that the heather pollen greatly predominated over grass. Consequently it may be inferred that where the turves were cut there was enough light to enable heather to flower freely; in other words, there was a clearing in the forest. This is the case in both the other Bronze Age analyses, though the heather dominance over grass is nowhere as great as it is in the present moorland.

This grass-heath vegetation stands in marked contrast to that found in similar circumstances on the lowland heaths and even on the Tabular Hills of North-east Yorkshire. The most obvious difference is the lack of weeds of cultivation, notably plantain (*Plantago*); under lowland barrows on heathland the plantain may comprise over 30 per cent of the non-tree pollen, and even where it is relatively scarce it is sufficiently well represented to indicate cultivation nearby if not on the site itself. Here, however, its occurrence is quite sporadic, much as in the moorland turf today, so we may conclude that these Bronze Age people were not agriculturists. Presumably, therefore, their way of life was pastoral. The clearings that they made became dominated at once by heather, which might provide grazing for sheep, especially if sufficient grass were associated with it. On the other hand, there are reasons for suspecting that these clearings were an undesirable result of their activity rather than its ultimate aim. For one thing, if the clearings had been heavily grazed they would probably have become grassy rather than heathy. Secondly, the fact that barrows were built in the clearings suggests that the land was no longer useful; in a similar way in the lowlands the barrows were apparently sometimes built on abandoned cultivated land. If this is so, the grazing must have been mainly along the forest margin, where there was sufficient light to allow grass to flourish, but insufficient to allow heather to invade. Just as we see this condition under birch on the fringe of a heath today, so similar conditions can be imagined under hazel in the Bronze Age.

To summarize, then, the sequences were probably as follows. Initially there was a dense forest cover of deciduous trees and shrubs in which oak, alder, birch and hazel were the main species. Mesolithic man lived in this forest and set fire to it, at any rate locally. This resulted in a lighter canopy, with some spread of heather and grasses beneath the trees, but also a change of floristic composition amongst the woody species themselves. Birch and hazel increased in prominence whilst alder decreased. There is no evidence of Neolithic settlement on these hills, so Bronze Age man was apparently the direct successor to Mesolithic man. When he came the forest showed little trace of earlier disturbance, but his own activities quickly modified it. It seems unlikely that he used fire, but rather through

grazing and perhaps felling he opened up the forest. This led to a big increase in the amount of hazel and eventually to clearings in which a grassy type of heath became dominant. These clearings were not cultivated and it seems likely that the culture was pastoral with the animals ranging in the forest or in the fringe of hazel scrub which was transitional between forest and clearings. The fact that the barrows were built in the clearings before they were very large suggests that the clearings were inadvertently rather than deliberately created.

No attempt has been made here to interpret the pollen analyses in terms of the relative abundance of the various species on the ground. Different plants produce widely different amounts of pollen, and whilst there are some guiding principles, a reliable picture in ecological detail is not possible. Nevertheless, perhaps sufficient has been done to present a general view of the changes which took place and to suggest the factors which brought them about. Even when given the facts it needs a prodigious effort of the imagination to envisage the Cleveland Hills in their forest guise, but this serves to remind us that the student of the past must not look at the past through the blinkers of present impressions. It serves to remind us even more of the enormous effect on our upland landscape which was exercised by small ephemeral populations a long time ago.

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NOTE ON THE BURTON HOWES

by Raymond H. Hayes

The Burton Howes (Nat. Grid Ref.: NZ 608033) are four tumuli on the 1400 ft. escarpment overlooking Greenhowe Botton (Old Norse 'Botn'—a hollow). They were all previously disturbed but no record of this disturbance exists. Mr Rowland Close re-examined them in 1955–6.

Barrow 1A was 45–47 ft. in diameter and 3 ft. in height, entirely constructed of turf apart from a few upright stones marking its perimeter. No trace of cremated bones was found; an inhumation would have vanished owing to the acidity of the turf.

Barrow 4D, the largest, was 50 ft. in diameter and 8 ft. high at the centre. A deep circular pit had been dug into the centre and the large stone cist, 4 ft. 6 in. by 3 ft. wide, was empty—apart from scraps of a large overhanging rim urn with corded lattice pattern on the rim. In the clay at the bottom of the cist were found a few crushed bones and flint chippings, and a clay bead, $\frac{5}{16}$ in. in diameter, green on a brown core. The secondary cremation (unaccompanied) was on the south-east side 3–4 ft. down. The mound had been raised after the insertion of this cremation and four large postholes and the stone casing (as at Loose Howe) were probably part of this alteration. Built into the turf mound were concentric kerbs of stone respectively 44 ft. and 38 ft. in diameter, the smaller one not being continuous.

Assuming that the urn was the primary burial, a mid-Bronze Age date 1400–1200 B.C. would be likely, but owing to the early disturbance we cannot be certain that the cist did not contain other vessels, or inhumations, of the Early Bronze Age.

SEUTHOPOLIS

Map to show the site of the Thracian city Seuthopolis, described by Professor Dimitrov (pp. 92–102). Seuthopolis, (42 40′N, 25 20′E) *is approximately 8 kilometres* (5 *miles*) *west of Kazanluk, centre of the famous rose-growing area* (rosa damascena) *which produces 'attar of roses'. The Šipka Pass, carrying the road through the Balkan Mts, is* 1,330 *metres. The R. Toundja is a tributary of the Maritsa. The map is based on the* Generalkarte v. Mitteleuropa 1 : 200,000 [Tarnowo 43/43], *enlarged approximately* 1½ *times.*

(*a*)

(*b*)

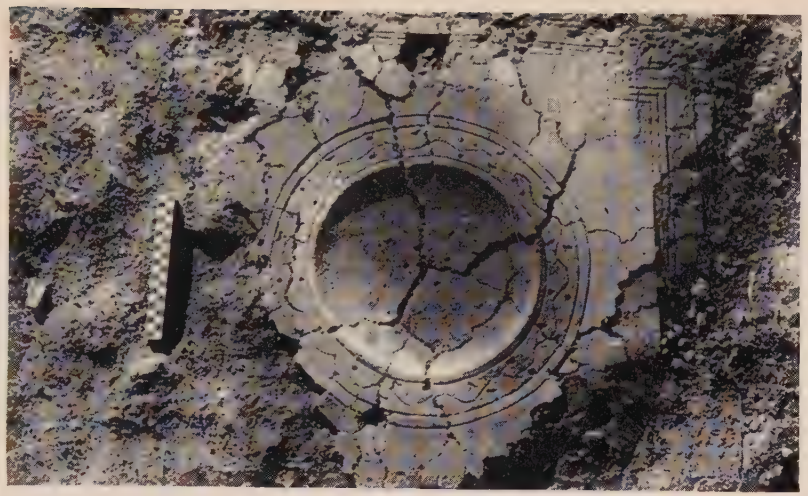
(*a*) *General view of the excavations at Seuthopolis.*

(*b*) *Ruins of the* pastas *house* (*for plan see Fig.* 2, *p.* 96).

(*c*) *Reconstruction of the* pastas *house.*

(*c*)

(a)



(a) *A hearth altar from Seuthopolis* (*See also Fig.* 4, *p.* 98).

(b) *Ruins of a cupola tomb, built of burnt bricks.*

(c) *A well built of bricks.*

(d) *An antefix from the palace.*

(b)

(c)

(d)

SEUTHOPOLIS

See p. 91–111]

(*a*)

(*b*)

FORGERIES

(*a*) *A Colombian pottery effigy vase, one of the finest modern fakes to come from Colombia.* 17 *in. high.*

(*b*) *A Roman-style cup from Egypt with electrotype Bacchus heads.* 5 *in. high.*

See pp. 103–6]

[*Photos:* (*a*) The Illustrated London News (*b*) John R. Freeman and Co. All photos by courtesy of the British Museum.

(*a*)

(*b*)

FORGERIES

(*a*) *Modern limestone head of crowned Buddha from Siam imitating a Khmer head of* 11–12*th century.* 13 *in. high.*

(*b*) *Chinese oracle bone of* 2*nd millennium* B.C. *inscribed with genuine archaic characters* (*bottom*) *and larger modern imitations* (*top*). 8 × 7½ *in.*

See pp. 103–6]

[*Photos :* John R. Freeman & Co.

FORGERIES

Two 'mermaids', part monkey, part fish. The top one (12 *in. high*) *is perhaps* 17*th century; its X-ray shows the wire structure.* (Top) *Lent by Sir Alistair Hardy;* (bottom), 14 *in. long, lent by Mr and Mrs M. V. Carey.*

See pp. 103–6]

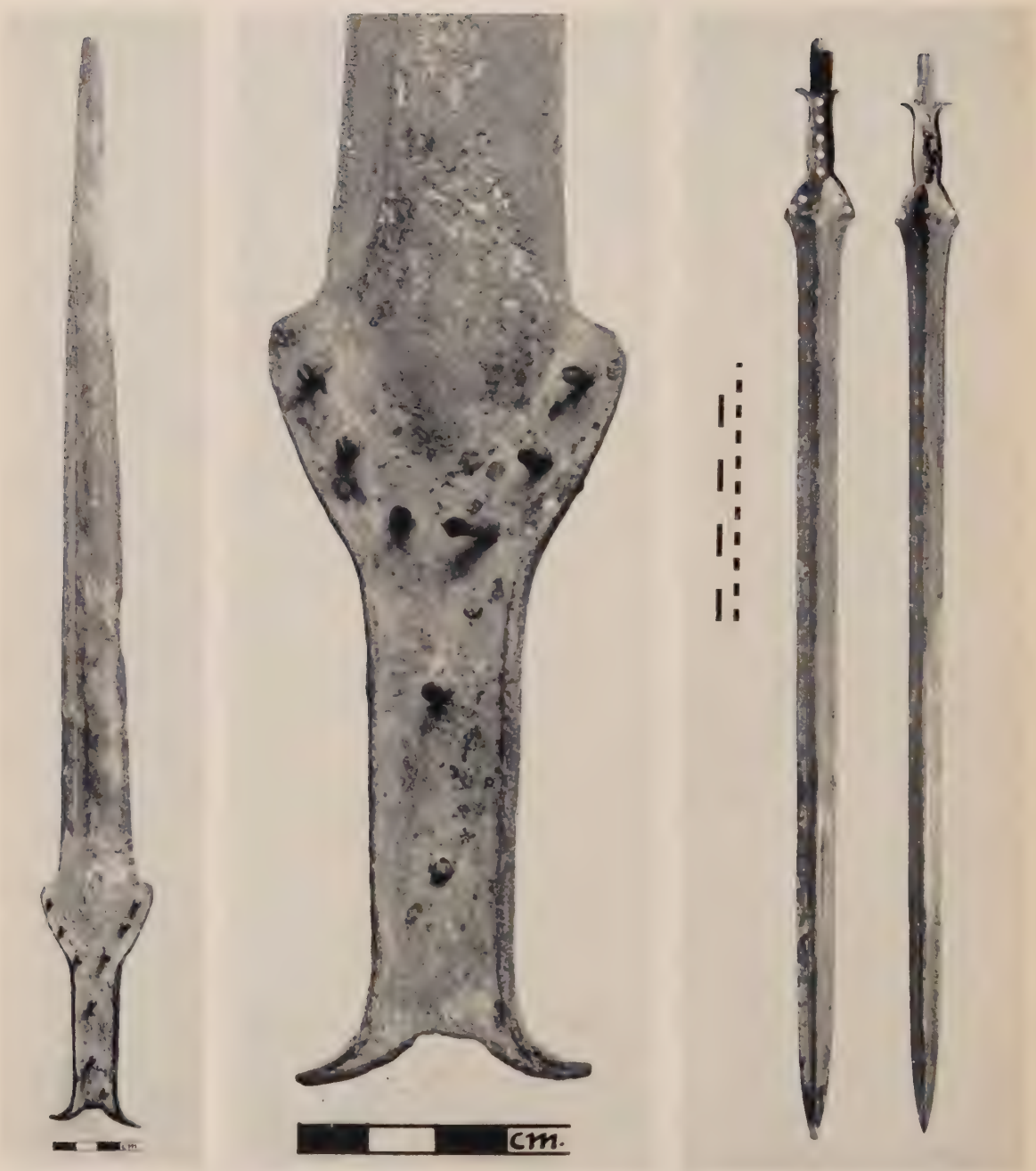
[*Photo:* The Illustrated London News.

(*a*)

(*b*)

(*c*)

(*a*) FORGERIES. *An 'Etruscan' sarcophagus alleged to have come from Caere, but made in Rome about* 1860. *Length* 5 *ft.* 2 *in.* [*See pp.* 103–6, *Photo: John R. Freeman and Co.*] (*b* & *c*) OVERTON EXPERIMENT. (*b*) *Antler pick used as* pick *in clean chalk;* (*c*) *Antler pick used as a lever. Figure on right is using a striking baton.* [*See pp.* 129–34].



(*a*) (*b*) (*c*)

A NEW BRONZE SWORD FROM CYPRUS

(*a*) *Naue II sword in the possession of Mr Evangelos Loizou, Famagusta, Cyprus.* (*b*) *Detail of the hilt of this sword.* (*c*) *Two swords said to have been found at Graditza, Thessaly, Ashmolean Museum Nos.* 1927, 1383 & 1384.

See pp. 115–22]

[*Photos:* (*a* & *b*) Cyprus Museum, (*c*): Ashmolean Museum.

A NEW BRONZE SWORD FROM CYPRUS

Detail of the hilts of the Graditza swords in Plate XVI (c)

See pp. 113–22.

[*Photo:* Ashmolean Museum.

THE LLYN FAWR HOARD

The upper side and underside of one of the pair of bronze cheek-pieces (No. 1, see also Fig. 2, p. 150) from the Llyn Fawr (Glamorgan) hoard, enlarged to show traces of wear around the central slot.

See pp. 149-51.

Photos: Eric Broadbent

THE LLYN FAWR HOARD

The underside and two views of the upperside of one of the pair of bronze cheek-pieces (No. 1) from the Llyn Fawr (Glamorgan) hoard; and the upper side of No. 2. Overall length of No. 1, 164 mm.

See pp. 149-51].

[*Photos:* Eric Broadbent.

(a)

(b)

(c)

(*a*) A MEDAL OF THE LAKE DWELLINGS. *A medal made of bronze from the Swiss lake dwellings. Scale almost* 3 : 1 [*See p.* 151. *Photo: National Museum of Wales*] (*b* & *c*) FOLLY IN COBHAM PARK. (*b*) '*Dolmen*', *about* 5 *ft. high, with shell of a giant clam beneath;* (*c*) *Sarsens and the Classical Temple in* '*Lady Darnley's Garden*'. [*See p.* 151–2.]

An Experiment in Field Archaeology

by PAUL ASHBEE and IAN W. CORNWALL

Recently a Research Committee was set up by the British Association for the Advancement of Science to investigate by experiment the denudation and burial of archaeological structures. The Committee consists of P. Ashbee, R. J. C. Atkinson, I. W. Cornwall, G. W. Dimbleby (Chairman), P. A. Jewell (Secretary) V. B. Proudfoot, and P. M. White (Assistant Secretary). Here two members of the Committee describe an experiment made on Overton Down in Wiltshire—an experiment in field archaeology made with antler pick, scapula shovel and basket.

IDEAS about the prehistoric past have for long been based upon pieces dug from the mounds and hollows which are the remains of man's activities. Only recently has it been clearly realized that monuments are continually being modified by nature and that evidence of weathering, denudation and silting can be as informative as the artifacts sometimes found in them. A recent conference[1] showed that there was relatively little exact knowledge of how such processes take place and, in particular, of their quantitative aspects. As has been recently emphasized[2] archaeological excavation is a means of investigating the truth of an hypothesis by destructive analysis. It thus emerges that any study of the action and interaction of natural processes affecting a prehistoric monument must clearly involve the converse, i.e. a monument must be built and studied, in all its component parts, under controlled conditions. Today an impressive and substantial ditch and bank, cut into and heaped upon the chalk of Overton Down in Wiltshire, stands a shining witness to the execution of this principle.

The work of construction was undertaken with hand tools, but not all of these were modern picks and shovels. A part of the bank was dug and heaped with primitive tools and appliances—antler picks, shoulder-blade shovels and wicker carrying-baskets. It has long been known (from the not infrequent discovery of examples apparently discarded) that antler picks were the primary digging tools of those who dug the ditches and heaped the long and round barrows, the causewayed camps, the henges and the cursuses, but precise appreciation of the real potentialities of antler picks was slight. Greenwell[3] and Pitt Rivers[4] commented upon the use of such implements, while the subject was pursued in some detail by Horace Sandars[5] in his paper *On the Deer Horn Pick in the Mining Operations of the Ancients*, among others. Curwen's accounts[6] of the Sussex flint mines further stimulated such studies as did Clark and Piggott's classic paper.[7] From Sandars's,

[1] Meeting of the British Association for the Advancement of Science held in Glasgow in 1958.
[2] R. J. C. Atkinson, *Archaeology, History and Science* (1960), 24.
[3] *B.B.* 37.
[4] Pitt-Rivers, *Excavations*, III, 135.
[5] *Archaeologia*, LXII, 101–24.
[6] *Prehistoric Sussex* (1929), 14–29; see also *Grimes Graves Report* (1914).
[7] ANTIQUITY, 1933, 166–83.

Curwen's initial and subsequent study[8] and from summaries of the past two decades[9] the view emerged that the antler picks were almost invariably used as punches and levers to break up chalk. Thus, such thought as has been given to prehistoric mining and digging has been to some extent inhibited by acceptance of that notion.

While modern use of antler picks, or of any other prehistoric tools, is bounded by the entire divorce of modern man from the relationship to external environment of the original users, here at Overton Down information was gathered which may, perhaps, modify our views as to their practical utility, plus quantitative assessment.

As antler picks, shed Red Deer (*Cervus elaphus elaphus*) antlers from Woburn Park and Broom Hall were of rather small size, so they were supplemented by a few antlers of the modern Wapiti (*Cervus elaphus canadensis*). Neolithic and later red deer were often much larger than their modern descendants, so this was felt to be justifiable. These wapiti antlers were preferred by many of the more active workers for their robustness and greater weight. The picks were prepared by removal of the bez and trez tines with a hacksaw[10] leaving only the brow tine. In some instances twisted brow tines were obliquely truncated to obtain a straighter working point.

At the outset of digging, a sarsen stone between 1 and 2 cwt. in weight was moved from its seating in the ditch, using tine and beam as a lever; a process which demonstrated to all the intrinsic strength of picks and the durability of antler. While antler picks may well have been used perforce as levers for breaking chalk in the confines of a flint-mine gallery, our modern men, who rapidly became attached to specific tools, found them to be most efficient implements for loosening soil and undisturbed chalk when swung *as picks*. After practice it was found that hewing from an arranged vertical face was the most effective method and economical of effort (Pl. XV (*b*)). Indeed, it was even possible to penetrate and loosen layers of flint in the same manner. Occasionally a pick had to be hammered into the chalk, when the brunt of the blows fell upon the back of the burr and the basal part of the beam. Hardwood billets and wooden mauls scarcely marked, and at the most only polished, the hard, relatively fresh antler. Bruising did result from the use as an improvised maul of a sarsen stone, but absence of bruising on a prehistoric antler pick does not therefore necessarily mean that the tool was not hammered with some softer beater. However used, no antler pick was broken during the operation, although the brow tine points wore and split on flints in a manner often noted on prehistoric picks.[11] Pick marks left on the chalk by our picks were again directly comparable with those observed in prehistoric workings.[12] In short, a selected *antler pick* in the hands of a skilled and perspicacious practitioner is an efficient, strong and durable *pick* which, at least in the open, is only hammered in and used as a lever when this is absolutely unavoidable (Plate XV (*c*)).

Most of the available scapulae for use as shovels were those of horses and, indeed, the few bovine examples were not much in favour on account of the more prominent acromion. An example from a small Indian elephant was also available but was noticeably avoided. Used as end-scrapers, the scapulae were found to be effective. Workers held the flat rectangular open baskets before them and drew their antler-pick-loosened spoil onto them or co-operated, one steadying a basket and another scraping. Like the picks, the worn edges of scapulae left distinctive marks on the chalk. The shoulder blade, used as a scraper

[8] *Archaeology of Sussex* (1937), 112–3; (1954), 96–130.

[9] S. Piggott, *British Prehistory* (1949), 72; J. G. D. Clark, *Prehistoric Europe: the Economic Basis* (1952) 174–80.

[10] On working antler: *PPS*, XIX, 148. It was felt that there was little to be gained by trimming the obdurate shed antlers with flint blades.

[11] e.g. Sandars, *op. cit.*, pl. XVI, 7; *Archaeologia*, LXXXIV, pl. XLVII.

[12] Pitt-Rivers, *Excavations*, IV, 104 (Allegedly the marks of a bronze palstave).

where space is unrestricted is, relatively, an inefficient implement. It was found that two shovellers and two carriers were needed to keep one skilled pick-man fully occupied. Although, however, these results were obtained in the open, it is possible that the use of scapulae as *shovels*—often handled[13] is an adaptation peculiar to the mines.

The flat, rectangular, open baskets, of a form based upon early examples[14] were not ideal for transport, except perhaps on the head, of the 30 lb. weight of chalk, which seemed to be the most economical load. None of the available female workers seemed to have the necessary skill for this, although perhaps it could have been acquired with prolonged practice. Notwithstanding difficulties, which modern enthusiasm overcame, the quantitative results with these baskets were not far short of those obtained with buckets. Leather buckets or ruck-sack-like scoops[15] should not be excluded from any future experiment, especially where dry sandy soils are involved.

Time and motion studies of modern methods were carried out under our conditions, i.e. with steel pick, shovel and bucket, working in humus and chalk. The results of these work studies of modern and *ancient* methods are set out below. A last sally on 16 July by ancient methods was undertaken by Dr P. Jewell and Mr P. Ashbee, with Dr I. W. Cornwall basketing (and weighing) and was a *Stakhanovite* operation in the nature of an attempt to determine a maximum possible speed over a short course. Such an output could not have been maintained over a whole day.

The results of the work studies are presented in Table I. This gives the output of various groups of individuals over short, timed, periods. A record was kept, however, of the overall effort expended in completing the primitive-tool section of the ditch and on average it was found that 2·6 cwt. of material had been moved per man/hour. This figure is to be compared with that of 3·6 cwt. per man/hour for the rest of the earthwork.

As was expected at the outset, the modern methods greatly surpassed the primitive in average efficiency by a factor of 3 or 4. The fitness, adaptability and physique of the worker together with soil conditions all played their part in determining differences. However, the difference in efficiency between antler pick *vis-à-vis* steel pick and basket *vis-à-vis* bucket were much smaller than had been anticipated. The *shovel* is the modern tool which advantages the modern hand methods; the scapula, however used, was a poor substitute. It is to be wondered therefore whether or not a more effective counterpart of the shovel was known in prehistoric times. Indeed, in addition to wooden shovels from Mid-European metal mining contexts[16] one has been claimed from a West Country barrow[17] but no definite details have been published. A blade capable of picking up loose material bodily in one movement is clearly the decisive factor.

From the tables it should be observed in the light of the foregoing that the best performance (Stakhanovitic) with primitive tools by three fit adults, over the short spell of 40 min., was only 8·2 cwt./man/hour, as against 16·5 cwt./man/hour achieved by two boys carrying their own buckets, over a full hour with modern tools. It emerges that a good average for practised and fit *primitive* workers in not too difficult ground was about 5 cwt./man/hour. The weight of a cubic foot of solid chalk was directly determined and found to be approximately 1 cwt. It can be seen, therefore, that material could be excavated and removed from a ditch by primitive methods at the rate of 5 ft.^{3}/man/hour.

[13] Curwen (1954), *op. cit.*, 100; *SAC*, LXVII (1926) 37–43.
[14] Caton-Thompson and Gardner, *The Desert Fayum* (1934), 43.
[15] J. G. D. Clark, *Prehistoric Europe: the Economic Basis* (1952), 221, fig. 123.
[16] Clark, *op. cit.*, 193, fig. 108, 2.
[17] *ANL*, II, 110–11; Evidence for the use of a shovel has been adduced by A. C. Thomas at Gwithian, Cornwall. See also Nationalmuseets Arbejdmark (1960), 107, fig. 9, 3.

TABLE I

MODERN METHODS

Date 1960	*Team*	*Duration of experiment*	*Buckets carried*	*Av. net bucket load (lb.)*	*Rate bucket/ hr.*	*Wt. spoil per hr. (lb.)*	*Cwt./ head/ hr.*
10th July	(1) 2 1 boy, 1 girl carrying own buckets	50 min.	106	30	127	3816	17·0
12th July	(2) 2 2 boys, carrying own buckets	66 min.	108	37·8	98	3711	6·6
		ANCIENT METHODS					
10th July	(*a*) 3 1 boy, 1 girl, 1 bucket carrier	(i) 25 min.	40	21	96	2016	6·0
		(ii) 65 min. (inc. (i))	74	21–27 (mean 24)	68	1639	4·9
12th July	(*b*) 5 2 boys, 2 girls 1 bucket carrier	(i) 40 min. (a.m.)	56	34	84	2856	5·1
		(ii) 70 min. (p.m.)	100	36	86	3086	5·5
13th July	(*c*) 3 Adults 1, 1 1 bucket carrier	73 min. (a.m.) (broken time)	58	37	48	1764	5·2
	(*d*) 5 2 girls, 2 adults 1 bucket carrier	55 min. (p.m.)	35	37	38	1413	2·5
15th July a.m.	(*e*) 4 2 adults, 1 boy 1 bucket carrier	(i) 103 min. (a.m.)	88	34	51	1743	3·9
		(ii) 200 min. (a.m.) (inc. (i))	144	34	43	1469	3·3
15th July p.m.	(*f*) 4 2 adults, 1 adult 1 bucket carrier	145 min. (broken time)	100	34	41	1407	3·1
			Baskets	*Av. Bask.*			
16th July	(*g*) 3 2 adults 1 basket carrier	(i) 40 min. (Stakhanovites)	56	33	84	2772	8·3
		(ii) 60 min. (Same team at ordinary pace)	56	33	56	1848	5·5

The foregoing figures have been applied to the approximate quantities involved in the construction of a number of prehistoric monuments.

A small bowl barrow[18] with a mound 40 ft. in diameter heaped from a ditch 6 ft. in width and 4 ft. deep, would, at average Overton Down rates of primitive construction, entail: 694 hours or 70 10-hour days, which would occupy 10 people for a week.

A large bell barrow[19] with a mound 140 ft. in diameter and a ditch 10 ft. wide and 5 ft.

[18] Snail Down: *WAM.*, LVI, 127–48; LVII, 5–8.

[19] Snail Down, *op. cit.*

deep would, however, need: 4650 hours or 387 12-hour days, but if people were allocated a 7 ft. length of ditch per person, then 70 people could work together and only 5–6 days would be required.

Whatever the social system and surpluses that sustained prehistoric workers for barrow building, there are specific massive monuments which are mute testimony to concentration and organization of labour upon a near-herculean scale. The original volume of solid chalk excavated from the Avebury ditch[20] was of the order of 3,400,000 cu. ft. If the man-output of about 5 ft.^{3}/hour determined at Overton Down were sustained, this vast quantity would entail 680,000 man/hours. Expressed in terms of 10 hour man/days, 68,000 would be the total, which could be expressed as 1000 men working for 68 days or 100 men working for 680 days—about two months short of two years.

Of the monuments of prehistoric Britain the Dorset Cursus[21] is the largest, and was until recently one of the least known.[22] Its parallel lines of bank and external ditch, with the ends closed by transverse stretches of earthwork continuous with the sides, pursue an undulating and slightly sinuous course across 6 miles of the Dorset downs, enclosing, thus, some 220 acres. The volume of the bank of this mighty work is between 5,000,000 and 5,500,000 cu. ft. even today, in spite of its denuded character. In its original state it may well have contained more than 6,500,000 cu. ft. of chalk. Such a bank would have entailed the excavation of the order of 3,700,000 cu. ft. of solid chalk from the ditch[23] which would have taken, again were the constant rate of 5 ft.^{3}/hour maintained, 740,000 man/hours. This in terms of 10-hour man/days is 74,000, which is equivalent to 1000 men working for 74 days or 100 men for 740 days—about 2 years.

Several difficulties arise, however, in the direct application of the Overton Down rates. First of all, there are the carrying and tipping factors—which would have been considerably more time-consuming on the greater monuments, not to mention preliminary clearance of the site. With experiment and record it should be possible to arrive at a ratio which is proportional to the difference in magnitude of certain works from a given experimental earthwork. Thus, at Avebury, the crest of the bank originally rose about 55 ft. above the bottom of the ditch, as against the mere 11 ft. of Overton Down, a ratio of 5 : 1. This is a vertical expression which takes no account of horizontal distance, i.e. traversing the berms. If therefore, with all these conditions in mind, the Avebury time (for 100 men), arrived at by application of the Overton rates, be merely multiplied by 5, a time of 9 years 2 months is reached. This figure compares well with a realistic estimate made recently,[24] based upon a standard output of about 4 cu. yds. of chalk per day from two pickmen. At this juncture it is convenient also to recall that it was observed that 30 lb. weight of chalk seemed to be the most economical basket load, notwithstanding difficulties of carrying and tipping.

It has been estimated[25] that to heap up Silbury Hill an operation entailing the transport of the equivalent of nearly 50,000,000 such basket loads had to be planned and put into execution. Secondly, in the estimates made so far, the work of the supervisory staff has not been included. It was found at Overton Down that they contributed 388 man/hours as against the 1167 man/hours worked by the diggers. On the other hand, members of the

[20] *Archaeologia*, LXXXIV, 99–162.

[21] ANTIQUITY, XXIX, 4–9.

[22] Although initially published by Sir Richard Colt Hoare, *Ancient Wiltshire*, II (1819), 33.

[23] An expansion factor of 1·75, i.e. as an approximation the solid chalk expands into buckets from 4^3 ft. to 7^3 ft.

[24] Atkinson and Sorrell, *Stonehenge and Avebury* (H.M.S.O. 1959), 59.

[25] *Ibid.*

supervisory staff took part from time to time in the actual digging work, both modern and primitive, but usually only for short periods. While many undertakings of the planners and supervisors at Overton Down, such as surveying and photography did not have their counterparts in prehistoric times, none the less a substantial contribution of planning and supervisory time to great works such as the Avebury ditch and bank, the Dorset Cursus and Silbury Hill must be envisaged. In addition, there is the question of the sustenance and housing of large labour forces who, for the duration of their efforts, one suspects, may well have been full-time specialists. As at Overton Down, this is a matter of yet more man/hours to be added to the grand total. Thus when all the social and economic factors are considered and added, the totals, for all monuments, in terms of the sum total of human endeavour, are surprisingly great.

This particular aspect of the Overton Down archaeological field experiment has, besides raising and directing attention to many problems, above all else engendered a more preceptive awareness of all that earthworks entail. In addition it has demonstrated directly and indirectly, when all the relevant factors are taken into consideration, including the inexperience of the modern users, the remarkable efficiency of the *primitive* tools. We may well tend continually to underestimate the engineering capabilities of our prehistoric ancestors.

Book Chronicle—*continued from p.* 122.

GOD'S WILDERNESS: Discoveries in Sinai. *By* Beno Rothenburg *in collaboration with* Y. Aharoni *and* A. Hashimshoni. *pp.* 196, 90 *photographs*, 16 *maps and plans. London, Thames and Hudson*, 1961, £3 3*s*. An account of a scholarly archaeological exploration of the Desert of Sinai through which the Children of Israel wandered.

I NUOVI METHODI DI PROSPEZIONE ARCHEOLOGICA ALLA SCOPERTA DELLE CIVILTA SEPOLTE. *By* C. M. Lerici. *pp.* 418 *on art paper with numerous diagrams, plans and photographs. Milan, Lerici Editori*, 1960, 3000 *lire*. A summary of the techniques developed by the Fondazione Lerici for the reconnaissance of Etruscan tombs from 1956 onwards.

WORLD PREHISTORY. *By* Grahame Clark. *pp.* 283, 12 *plates*, 7 *maps. Cambridge, the University Press*, 1961, 30*s*. *bound*, 15*s*. *paperback*. A brief outline of the prehistory of the whole world.

ANCIENT EGYPT: A Cultural Topography. *By* Hermann Kees. *pp.* 392, 24 *plates*, 11 *text maps. London, Faber and Faber*, 1961, 42*s*. Originally published by Klotz Verlag as *Das Alte Aegypten*, now translated by I. F. D. Morrow, and edited by T. G. H. James.

THE CRETAN COLLECTION IN OXFORD: THE DICTAEAN CAVE AND IRON AGE CRETE. *By* John Boardman. *pp.* 180, 48 *plates, Oxford, the Clarendon Press*, 1961, £5 5*s*.

3,000 YEARS OF DECEPTION IN ART AND ANTIQUES. *By* Frank Arnau. *pp.* 349, 48 *plates. London, Jonathan Cape*, 1961, 35*s*. A translation of the German *Kunst der Fälscher der Kunst* (1959).

ANTIQUITY XXXV, 1961

The Find Places of the Knossos Tablets

by L. R. PALMER

In the last number of ANTIQUITY *we published an article by Mr Sinclair Hood, Director of the British School at Athens on* 'The Date of the Linear B Tablets from Knossos' *and explained that we had invited Professor Palmer to reply. We now print Professor Palmer's answer as an article. Mr J. R. Boardman has agreed to add further comments on this controversy, which we will print in the September number of* ANTIQUITY, *and Mr Sinclair Hood will reply in the December number.*[1]

MR S. HOOD'S article on the date of the Knossos tablets (ANTIQUITY 1961, 4) is much to be welcomed as providing a further opportunity of removing misconceptions. In view of the magnitude and the contentiousness of the issues it is fortunate that the question can be reduced to one of simple physical location. No scholar can date a tablet from its physical appearance. There is no such thing as an LM II tablet. This is a pottery classification. The chronology is determined by the ceramic associations. Thus we simply ask where and in conjunction with which particular pieces of pottery these inscribed lumps of clay were found. A simple pattern of enquiry is imposed, which, I hope, will be followed by future contributors to the discussion. I first identify the particular deposit by their *Scripta Minoa II* numbers and then quote my sources for their location.

First a word about sources of information. Until last year we depended on:

I (*a*) the excavation reports; (*b*) the *Palace of Minos* (especially vol. IV [1935]); and (*c*) the summary find-spots given in *Scripta Minoa II*. These I shall call '*the Reported Facts*'. We may now add:

II (*a*) the Daybook of the Knossos Excavations by Duncan Mackenzie; (*b*) the Handlist of the Linear B tablets compiled by Sir Arthur Evans currently during the excavation. I identified this document early in September 1960 among the papers of Sir John Myres in the Ashmolean Museum. It consists of three volumes of drawings in the form of a paste-up of cuttings from earlier notebooks. At the end of the first season Evans wrote in *BSA*, VI, 58[2]: 'I have copied over nine hundred of these tablets. . . . No effort will be spared to publish the whole collected material at the earliest possible moment'. It has proved possible to identify the cuttings from this 1900 notebook and to restore it to its original order virtually without lacunae. The notebook, of the same paper as the private notebooks of the same period (see next item), contains 917 drawings with indications of the find-spots in Evans's own hand. The order of entry follows in the main the order of excavation. By combining the data with those offered by II (*a*) it is possible to make archaeological determinations of great precision affecting the great bulk of the tablets.

(*c*) Valuable supplementary and confirmatory evidence is afforded by the third new source—the private notebooks of Evans, in which he noted, and often drew, objects which

[1] The following abbreviations have been used throughout this article: *BSA, Annual of the British School at Athens; DB,* Daybook; DM, Duncan Mackenzie; *SM, Scripta Minoa;* and *POM, Palace of Minos; SMK, Guide to the Stratigraphic Museum at Knossos.*

particularly attracted his attention. Among these are some inscribed sealings. The group of sources II I shall call '*the Recorded Facts*'.

In the present article I propose to offer a sample of the new evidence as it bears on the northern deposit of tablets, and in particular on the Room of the Stirrup Jars (RSJ). Mr Hood concentrates on this room, and this is reasonable since from first to last Evans focused attention on it as providing decisive stratigraphical evidence for the dating of the tablets.

We now follow our proposed schema of enquiry. Which tablets? At once we face a difficulty, for Evans lumps together the tablets found in this room and those found in the adjoining space to the east—the Room of the Saffron Gatherer. The former is in fact merely the inner recess of the latter and the tablets were treated as a single deposit: 'These inscribed fragments [in the RSG] belonged to a deposit, of which the bulk was found under the further wall of a little chamber to the west of this. This inner chamber has received its name from the "Bügelkannes" . . .' (*BSA*, VI, 44). This echoes an entry in DM/DB 4 v 1900: 'In the same region and extending along what seems to be the W wall of the space many fragments of inscription tablets in a very crumbling condition were found on the clay floor.' The articulation of the two rooms had emerged clearly by 8 May, and DM then entered the finds on a sketch plan. Next day the room was completely freed of deposit down to the underlying cement floor, thus destroying the stratigraphic evidence once and for all. It is fortunate that Evans himself drew a sketch plan of the same area, which is in complete agreement with DM. Both show the tablets alongside the stirrup jars with no hint of a stratification, and DM's words 'on the clay floor' put the matter beyond doubt. Such were the observations and records of 1900. The tablets in this deposit are SM 749–768.

In 1901 some further finds were made in the same area. 'At the foot of the N face of the N wall of this room and beneath the level of the floor were found yesterday several fragments of inscription suggesting a deposit, several being whole. . . . The mid N wall had collapsed and this accident facilitated the search for inscriptions' (6 iv 01). In the adjoining RSG, the northern part of the floor being found incomplete, this was removed on the west side and some fragments of inscription were found above an earlier floor ·40 lower. DM writes: 'The conclusion is that the earlier floor as well as the later is Mycenaean.' This was confirmed in 1923 (see below).

The results were briefly reported in *BSA*, VII, 35 with reference to 'another Mycenaean pavement'. Nothing was said about the position of the tablets found the previous year. In fact the 'N face of the N wall' is the south wall of the adjoining room, and DM's conclusion (summarized in Evan's own private notes) shows that he had no thought of adjusting the clearly recorded data. But this is Mr Hood's account of the matter: 'But in the following year, 1901, fresh evidence came to light, including fresh evidence about the tablets in question found near the north entrance. Following the collapse of a mud-built wall of the "Re-occupation period" it was seen that these tablets were actually resting on an earlier floor than the "Re-occupation period" vases.'

This is the crux of the matter. What is involved is a question of observed fact. In view of the issue of 'faking' raised by Mr Hood one has a right to expect more than ordinary care in establishing the facts. As it is Mr Hood has used a form of words not justified by anything in the report for 1901. As we have already seen the RSJ had been completely freed of deposit the previous year and the tablets had been transferred to the museum. How then was it possible for it to be *seen* that the tablets were resting on the earlier floor? What Mr Hood means is that the tablets of 1900 were *inferred* to have been on the earlier floor. But even this would be inaccurate in relation to the report for 1901. Evans makes no such state-

ment. It was not until the end of the fourth season that this adjusted picture of the stratigraphy in the RSJ made its appearance (FIG. 1).

This apparently convincing piece of stratigraphy requires comment. In the first place, as we have said, the relative position of the tablets is presented as *observed* when in fact it was 'reconstructed' and retrospectively adjusted. I regard this procedure as wholly inadmissible as a mode of scientific reporting. Mr Hood has no such misgivings. But even

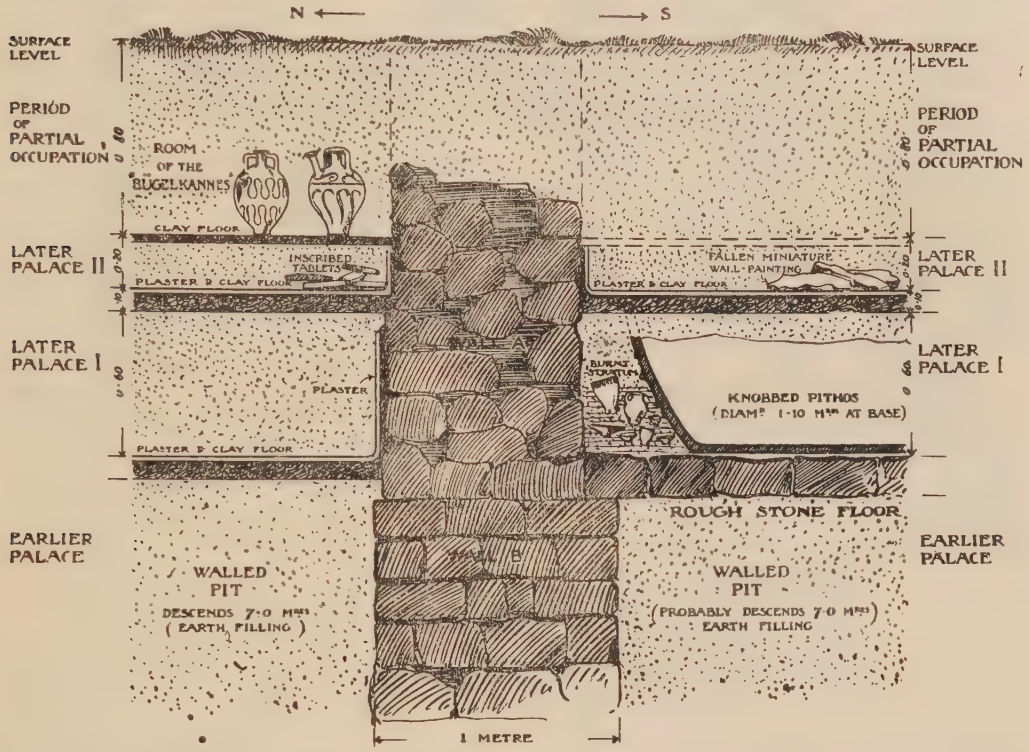


Fig. 1. Evans's section showing the stratigraphy in the Room of the Stirrup Jars (looking East), published in BSA (1902–3) IX, 26, Fig. 13, entitled 'Section above Deep Walled Cells showing later Floor Levels'.

if the facts had been observed as presented, this still would not have proved the LM II dating. For although the lower floor is labelled 'Later Palace II' (= LM II), no pottery had been found on it which justified this attribution. In fact later investigations in this area were to show that the lower floor also belonged to the same archaeological epoch as the upper clay floor on which the tablets had been found in 1900.

These results were obtained in 1923 and though unreported they have left their traces in our FIG. 2. I begin with the 'later wall' running S–N. This rests on a wall of the 'Early Keep'; 'but it slants in a way which betrays its late character, and it may even be a construction of the Re-occupation Period' (DM/DB 1923, II, 4). The wall consisted of limestone blocks, but at the north end there was a gypsum block resembling a door-jamb with dowel holes at each end. It was placed transversally with an interval between it and the nearest limestone block of 24 cm., which was filled with clay mortar. The pavement was

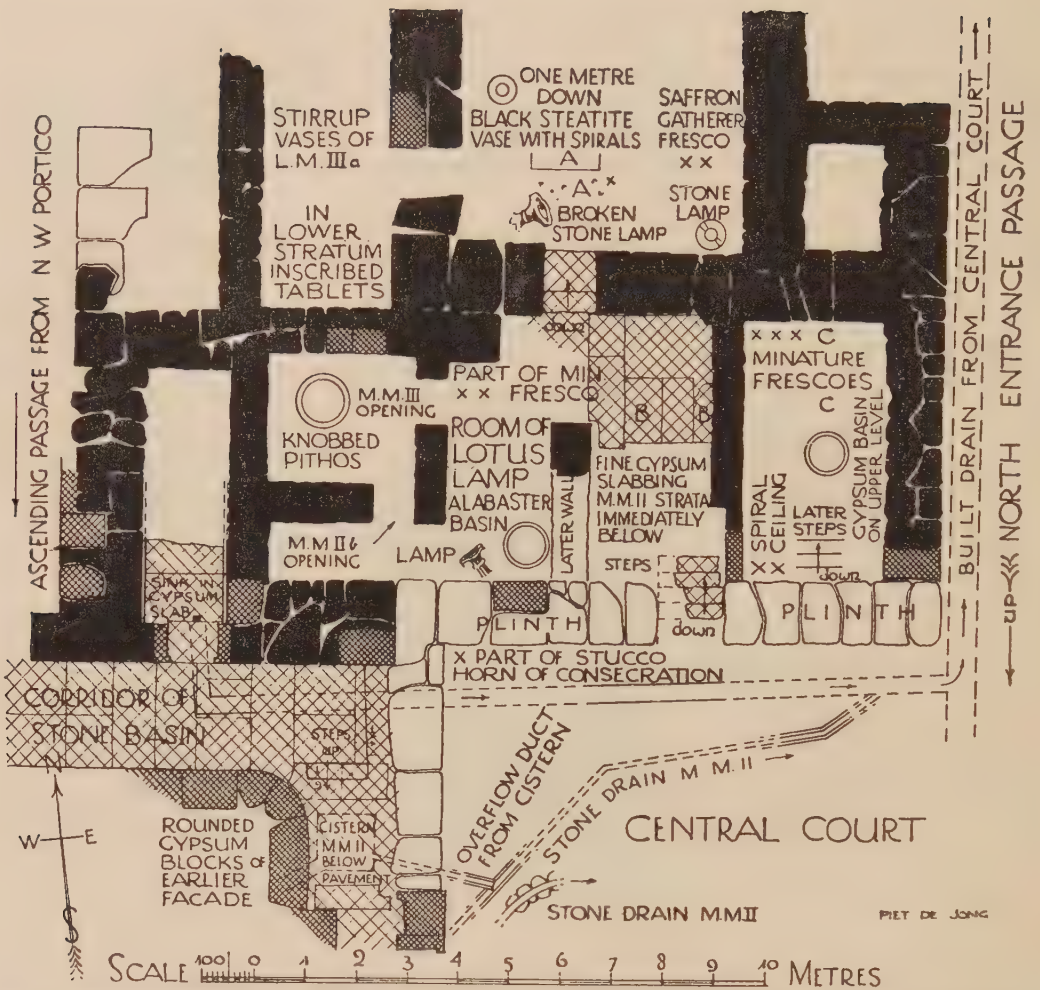


Fig. 2. Evans's figure 9 from POM, III, *19, entitled 'Part of North-West "Insula", showing Position of Principal Finds'.*

continuous underneath it, but the greater part of the gypsum block rested on another such block also resembling a door-jamb and slanting in the same way. 'All this goes to confirm the late character of wall 6' [= the slanting wall].

The deposit was now removed on the west side of the underlying gypsum block. 'A little distance away from the end and ·32 below the level of the paving alongside on the north three fragments of a LM II pithos came out in the deposit. This would indicate that the slanting wall was built in the Re-occupation Period when Late Minoan pottery of the end of the Palace Period (LM II *b*) had already got stratified' (*ibid.* p. 9).[2]

[2] Evans in his discussion of this wall (*POM*, III, 17) states that the separation into two rooms by a dividing wall dates from an advanced stage of MM II, quoting pottery of the fully developed MM II class found on both sides of the wall. He refers to excavations of 1928, of which I have not been able to find any record.

[*continued on p.* 139

I now consider the doorway opposite the north end of this Re-occupation wall. The matter is of such importance that I anticipate full publication of the DB material and quote it in full.

'Very noticeable is the gap opposite the slanting wall 6. This interval was evidently made through the removal of a block or blocks of the wall. Thus a doorway was formed, which communicates into the next space on the north side. But there are no door-jambs and though there is a gypsum threshold, the plaster curving up at the sides suggests the same Re-occupation Period as the contents of the "Room of the Stirrup Vases". This stucco or lime-plaster takes the place of the gypsum paving west of the doorway on the south side, but in the doorway itself it steps down North ·08 below the level of the pavement and so fits on to the edge of the gypsum threshold there. From the north edge of the threshold again the plaster curves down and joins on to a plaster floor on the north side lying ·13 lower than the threshold. This plaster, which is of a pale colour, occupies the South-West corner of the room and fits on to a gypsum slab towards the centre of the area. The gypsum slab may be part of an earlier arrangement while the stucco fitted on to it looks like the work of the same Re-occupation people who possessed the stirrup-vases found in the next room. The stucco passes directly from wall to floor with an angle which is rather sharp from one to the other. The stucco is preserved on the south and west walls for ·07–·08 from the floor and is in two layers each ·02–·03 thick, pointing to repeated repairs. . . .

'This Re-occupation floor-plaster resembles more closely that found on mainland sites like Mycenae and Tiryns than that of the Light Areas of Knossos.'

This then is the excavation history of the Room of the Saffron Gatherer. The tablets were found on the clay floor. Underneath was '(1) a good white cement floor 1 metre below the surface, (2) another cement floor 40 centimetres below the first with a large slab embedded in it' (*BSA*, VII, 35[1]). It was on this third floor down that DM found the successive layers of Re-occupation plaster.[3]

It remains to add that in 1928 a sounding was made under the pavement of the Central Court to the west of the northern entrance passage bordering the suite of rooms we have been discussing. Among the sherds recovered were some of LM III date, showing that the pavement must have been laid when these sherds were stratified (*SMK*, I, II, 1).

We are still not at the end of this story. Which tablets did Evans say he found in the RSJ? His final version is to be read in *POM*, IV, 733 ff. in a section headed: 'Hoard of Tablets referring to Painted Clay "Stirrup Vases" from Area above "Early Keep". Its Stratigraphic Relations.' This is the account which I, as a non-archaeologist, had always taken to be decisive. There is a catch-heading on p. 736: 'Decisive evidence for relative dating of "stirrup vase" tablets.' By way of illustration Evans reproduced the best preserved of this hoard in FIG. 719. This is tablet *SM* 700. We now know from his own Handlist, confirmed by the investigations of Bennett, Ventris and Chadwick in the Heraklion museum, that this is in fact the only tablet of its kind. There was no hoard

note 2 continued].

But there is in any case no mention of the very precise data recorded by DM in 1923 as above and in particular no awareness of the LM II *b* pithos although DM in his discussion of the stratigraphy especially singles out 'the presence of the LM II pithos fragments alongside a slab of pavement ·32 down' as one of the facts 'to be kept in view'. It remains to add that Evans altered the sketch plan in his note book of 1900 (dated 4 May), re-inking the gypsum block and correcting its position with the annotation against it 'later'. His own records, in so far as I have been able to trace them, thus agree with the above. On the reoccupation plaster see below.

[3] Evans assigns the plaster floor to the Great Restoration at the end of MM III. He adds that the 'late wall' has been removed (*POM*, III, 22). His marginal comment in DM/DB 1923 reads: 'later shown to be MMIIIb'; but we are not told how.

anywhere. Moreover, the Handlist gives the find-spot of No. 700 as 'W. Area: Nr. SW Entrance'!

What now of the diagnosis of the pottery in the RSJ? In *POM*, III, 22 the vessels belong to the 'earlier phase of the "Re-occupation period" (LM III *a*)'. But, in *POM*, IV, 736 'the ceramic remains here . . . mark the extreme limit of its re-occupation by the later squatters'. We find this vacillation at other key points of the exposition. Apropos of the Sword Tablets, which Evans in *SM*, I, firmly assigned to the Re-occupation period, his latest account (*POM*, IV, 854) sites them precisely 25 cm. below sherds of the mature LM III *a* class. This is corrected in V, p. 162: 'erroneously "LM III *a*". (A.E.).' The most extraordinary example concerns the important 'School Room' complex in the Domestic Quarter. Here the pottery was first diagnosed as MM III *b* (*POM*, I, 367, fig. 266). In *POM*, III, 265 ff., however, Evans acknowledged that he had made a fundamental error. The pottery under the partition wall in the corridor and in the School Room itself was LM III *b*, and sherds of this late class were found even under the threshold slab. Despite this, the revised FIG. 183 has the annotation 'blocked in LM III *a*.'

Thus the reported 'decisive stratigraphy' of the RSJ is discrepant at all points with the recorded facts. Those who share my own feeling that some tinge of doubt attaches to it may care to make the experiment of examining the detailed 'reported statements' concerning finds of tablets, ignoring the RSJ. These tablets are in any case few in number and form merely the fringe of an extensive deposit concentrated mainly on the western side of the northern entrance passage. This approach has methodological advantages. Shall we begin with the massive core of this northern deposit, where all sources agree, or with the fringe, where all is in doubt? For in the NEP we find a clustered mass of tablets, the 'Great Deposit', with the remains of the gypsum chests which had contained them and associated sealings which had been attached to the cord around the boxes. Entangled with them were some forty double vases, a large proportion of them practically perfect, and 'specimens of the same kind had already been found in the Room of the Bügelkannes' (*BSA*, VI, 51). Thus the ceramic associations of the Great Deposit are unchallengeable, and incidentally the same as those *recorded* for the RSJ. Evans had also found the first 'couple-amphora' of this type early in the excavation, again in a Re-occupation deposit (*BSA*, VI, 8). If any proof were needed that all these tablets belong to one deposit, it is given by Evans himself, who notes (*ibid.*) that 'chariots without wheels seen on some of these tablets bring them into connexion with the small deposit found on the floor of the Spiral Cornice Room just above the roadway'. Add now that SM 1009 (NEP) joins with SM 789 (RSC).

The tablets of this massive northern deposit, which must be treated as a unit, are 749–1054. The LM III *b* ceramic associations are beyond doubt. I repeat: this is the only way the tablets can be dated archaeologically. I am content merely to establish and note these associations, again with the warning that the stratigraphic diagram *POM* III, 171, fig. 114 enters the level of the roadway on which the tablets were found as LM III *a* despite the diagnosis of the couple-amphorae as marking the extreme limit of the re-occupation by 'squatters'. Such ceramic associations are the primary physical data on which all chronological determinations must rest. Attempts to save the LM II dating must necessarily take the form of complex hypotheses to explain away the primary data. But such attempts must face disqualification unless the scholars concerned first produce evidence of a massive deposit of tablets, with containers and accompanying sealings, entangled with whole pots of LM II type. Not until then can they even start on an equal footing. I have found no such evidence. Mr Hood has offered none. I am glad to note that he is prepared to concede that some of the tablets may be LM III, as Evans himself declared. For me this admission is enough. Those who are familiar with the Linear B material know that the unity of the ar-

chives can be established by internal cross-references. Identifiable individuals are mentioned in deposits in different parts of the palace.

In conclusion I repeat that this article offers no more than a sample of the new evidence. The mass of detail can be encompassed only within the framework of a book. When this is published scholars will be able to judge for themselves. I correct now a few inaccuracies in Mr Hood's article of a more personal kind both in the interests of truth and because they are calculated to cause prejudice and cloud dispassionate discussion. In the first place it is not true to say that 'until now Professor Palmer has only advanced arguments for a later dating of the tablets in newspaper articles and on the B.B.C.' My results were first submitted to the most appropriate of forums—the Minoan Linear B Seminar of the University of London Institute of Classical Studies on 1 June, 1960. About this I informed Mr Hood by telegram and he received the full 'Minutes' soon after. He should not imply that his only source of information comes from the public press. Then, again, it is not true to say that my views about the date of the tablets were first voiced in 1960 and that I had been anticipated by Professor Blegen (*Minoica*, 1958). The 'Minutes' already quoted contain an extract from an article I published in 1956: 'the historian is faced imperatively with the problem of the alleged 200 year gap between the fall of "Linear B" power in Crete and that of the mainland. At this point I shall do no more than repeat the question I put to an Oxford audience in August 1955: was the fall of "Achaean" power in Crete roughly contemporary with that in Pylos and was the same set of forces at work in both cases? In plainer language, were both disasters the work of the Dorians?' (*Minos* IV, 1956, 127.) It is, of course, gratifying that there should be this convergence of philological and archaeological results. We may now add the important article by Professor D. Levi (*Parola del Passato*, LXXI, 1960, 82–121), in which, after severe criticism of Evans's mode of archaeological reporting, he comes to the conclusion that in Minoan archaeology we must wipe the slate clean and begin all over again: 'bisogna rassegnarci a far *tabula rasa* di quanto ci è stato insegnato finora e ricominciare si può dire dal bell' inizio' (p. 120).

In so far as the tablets are concerned this means simply to establish the find-positions.

Levi poses a fundamental point of testimony to which we can give concrete exemplification. He alludes (p. 95) to the 'notorious difficulty' of forming a clear picture of the archaeological history of Knossos due to the fact that Evans in the *POM* presents his general framework of Minoan culture as established without submitting the evidence to the reader ('per un processo interno avvenuto nella conscienza dell' autore ma che quasi mai è riferito al lettore'). We have quoted the precise data recorded for the Room of the Saffron Gatherer. This is what is set before the reader of *POM*, III, 22, apropos of DM's Reoccupation plaster: 'There is considerable evidence that at the time of the great Restoration new plaster floors were laid down . . .' If there was considerable evidence for this drastic up-dating, why was it not presented? Scholars must here make their choice explicit. Are they ready to dismiss the precise data of the expert archaeologist who was employed because of his experience in keeping archaeological records? Will they prefer, with blind faith, to accept Evans's wholly incompatible and unsupported conclusion based on evidence which he wished to save the reader the trouble of examining ('che vuol risparmiare al lettore il tedio delle sue minuziose esperienze', Levi, p. 95)? Or will they subscribe to the scientific principle of *nullius in verba*? For my part I merely note the incompatibility of recorded and reported facts and console myself with the fewness of the tablets concerned in the combined RSJ + RSG (Nos. 749–773) as opposed to the univocal testimony about the Great Deposit.

Notes and News

NIEZWISKA AND THE CUCUTENI CULTURE

The last number of ANTIQUITY contained two interesting articles discussing, among other subjects, the painted pottery culture of Cucuteni. Here I am only concerned with two statements which require modification in the light of the excavations in which I took part and which were conducted by the late Professor Leon Kozlowski at Niezwiska in the province of Horodenka now part of the U.S.S.R. but then in South-eastern Poland.

Dr James Mellaart, in quoting the important new radio-carbon date of 3130 ± 80 B.C. for Cucuteni A, states that that culture 'equals Tripolye B1 and part of the next phase'. Professor Stuart Piggott remarks on 'a differential distribution-pattern between the settlements of Cucuteni A and B . . . the sites of the earlier phase being on the spurs of low hills and often defended by banks and ditches across the root of the promontory, while the Cucuteni B settlement is that of undefended valley settlements'.

The Niezwiska evidence agrees with neither of these general statements. The village of houses with pottery and tools like those of Bilcze or Cucuteni B was on the plateau above the Dniester valley though we did not excavate a sufficient area to decide whether the settlement was defended or not.

The Cucuteni A village was apparently undefended except by the river and lay on the bank of the Dniester at the mouth of the narrow valley in which the modern village stood. Cucuteni A and Tripolye A pottery were intermingled at the same level, and to prevent any misconception that this was due to telescoping of the strata we found one sherd (now in the Museum of Archaeology and Ethnology at Cambridge, England) that was adorned in the incised Tripolye A technique outside but in the painted Cucuteni A technique with both spirals and meanders inside. ('Excavations at Niezwiska, Poland, July 1926', by R. W. Hutchinson and J. P. Preston, *Liverpool Annals of Archaeology and Anthropology*, vol. XVII, nos. 1 & 2 and pl. III; see also C. F. C. Hawkes *The Prehistoric* Foundations of Europe p. 99). I am not of course, suggesting that Cucuteni A = Tripolye A entirely but merely that the two cultures certainly overlapped in time, and that the former overlapped with the Larissa culture in Thessaly (V. Milojčić, *Jahrbuch d. DAI.*, 1959, 57).

R. W. HUTCHINSON

CIVITAS—A FURTHER COMMENT

I owe an apology to Mr Frere and to readers for not discussing this subject in greater detail in ANTIQUITY, XXXIV. In trying to be brief and to the point, I did not make it clear that the vital evidence is small in quantity. Digressions on size of territory (cf. Nicaea in Bithynia or the Trajanic cities of Thrace), untechnical use of *civitas* to describe non-Roman communities (what other word could Caesar or Tacitus use?) and so on are irrelevant. I make no apology for being 'legalistic' on the technical question of whether a place like Calleva ranked merely as a *vicus*. The crucial points are these:

(1) Agedincum. I.L.S. 7049 distinguishes the *actor publicus* of the *civitas* from that of a *pagus* on the *territorium*. Other *pagus* dignitaries known to us are mostly *magistri*, but I.L.S. 5614 gives an example of an *aedilis pagi*. The aediles of the *vikani Agedincenses* (that is, the intramural inhabitants of the city itself, as e.g. I.L.S. 6663 shows) were so titled because this was the only form of words that would distinguish them clearly not only from the higher ranking (? 'curule') aediles of the *civitas*, but also from the (probably equal ranking) aediles of the *pagi* of Senonian territory. (I.L.S. 6659–60, distinguishing

curule and plebeian aediles at Arinum, are probably relevant here). The large territories of the Gallic cities no doubt made this multiplicity of magistrates and officials necessary (cf. the toparchs and *episkopoi* of the larger Greek city territories). For the need to allocate aedilician duties geographically, it may be relevant to note Dio LV, 8, Suet. *Aug.* 30, I.L.S. 4914, 6085 vv. 22, 33, etc. I.L.S. 7049 does not prove that Agedincum was merely a *vicus*.

(2) Divodurum Mediomatricorum. Mr Frere in note 26 points out that I.L.S. 4818 records a *vicus* of Divodurum: the inscription was set up by the *vicni vici Pacis*. These people cannot be merely the residents along a single alleyway (*vicus*), as reference to Festus s.v. *vici* explicitly shows. The *vicus* must be a constituent ward of Divodurum. But if Divodurum was itself a *vicus*, as the theory demands, then the *vicus Pacis* was a *vicus* of a *vicus*. This I believe to be nonsense.

(3) Petuaria. (*a*) There is no evidence for the existence of a 'civitas Parisorum'. (*b*) Military control will not have ceased merely because the garrison left: Petuaria was in a military area, and will have remained under military control until some other arrangement was made. (Not all *vici* in military areas were extramural settlements). (*c*) It clearly remained a *vicus* at least until the reign of Antoninus Pius (change of site is irrelevant, cf. e.g. I.L.S. 6092), but its later history is quite unknown. There is no evidence here bearing on the status of *civitates*.

Patient search will reveal nothing else relevant to the central issue in Mr Frere's article, nor have I noted any other evidence myself. But the least we can do is to start from something better than the definition of a city quoted by Mr Frere in note 25. Surely this is a slip?

Mr Frere's references to Roman statecraft are apposite. Dr Baradez is worth quoting —'rigidité de conception, souplesse de réalisation'. The idea of a city was a fairly 'rigid conception'. Flexibility was introduced *within* that conception, I think, not by creating something different. Until good evidence to the contrary is produced, I see no reason for thinking that the cities of Gaul and Britain were in any way abnormal.

J. C. MANN

C14 DATE FOR THE MALTESE EARLY NEOLITHIC

The first C14 date for a prehistoric phase in Malta has recently been released by the British Museum Laboratory. This date was obtained from a sample of charcoal which was recovered from below the floor of the smaller of two megalithic temples at Ta Haġrat, Mġarr during the excavations which I conducted there in the spring of 1954. Both typologically, and on the stratigraphical evidence recovered in these excavations, this building seems to be the earliest surviving megalithic monument in the Maltese islands.[1] The charcoal came from an undisturbed level containing sherds of the IB (Mġarr) phase, associated with some with decoration apparently of early IC (Zebbuġ) type, and just above the floor on which the wall foundations were resting. It should, therefore, represent a phase just after the erection of the building itself, and perhaps late in the IB phase. The date obtained (BM 100) is 2700 ± 150 B.C. (4660 ± 150 B.P.).

This seems to agree with the general trend of C14 dates for the European Neolithic, being somewhat higher than would have been expected a few years ago on the archaeological evidence alone, though the discrepancy is not as large as in the case of some of the dates for sites in temperate Europe. My guess-date for this phase in 1953 was *c.* 2100 B.C., so that it is of the order of half a millennium rather than a full one.

It is perhaps surprising that the date for the earliest Maltese megalith should be

[1] Evans, *Malta* (Thames and Hudson), 1959, 51 and 88 ff.

appreciably later than some obtained by the same method for Western Neolithic material and megalithic monuments in N.W. France.[2] On the other hand, it agrees well enough with dates in the first half of the 3rd millennium B.C. obtained for Cortaillod in Switzerland and Chassey in Southern France.[3] It would also fit well enough with the later 3rd millennium date recently obtained at Los Millares,[4] which would now seem to be at any rate partly contemporary with the IC (Zebbuġ) phase in Malta, which is characterized there by the first appearance of V-perforated buttons, anthropomorphic decoration on the pottery, and the Zebbuġ statue-menhir.

The date also supplies a confirmation of Professor Bernabò Brea's views on the need for re-dating at any rate the earlier phases of Period I in Malta,[5] though I think that the archaeological arguments for bringing phase IE down to *c.* 1500 B.C. are still strong.

In view of the high datings obtained for impressed wares in the W. Mediterranean, however, it is remarkable to find later developments apparently lagging so far behind those in Central and East Europe[6] and is probably an indication that we do not yet fully understand the earliest phases of the Neolithic in this area.

JOHN D. EVANS

[2] Giot, ANTIQUITY, 1960, 147–8.
[3] e.g. Tauber, *Science*, 126, no. 3227, 4–5.
[4] ANTIQUITY, 1959, 239.
[5] ANTIQUITY, 1960, 132–7.
[6] Waterbolk, ANTIQUITY, 1960, 14–18; Mellaart, ANTIQUITY, 1960, 270–78.

PLATE XX(*a*)

A MEDAL OF THE LAKE DWELLINGS

Recently shown at the National Museum of Wales, and since presented by Alderman T. Owen Jones of Old Colwyn, was a medal (PLATE XX(*a*)) of bronze from the Swiss lake-dwellings—presumably those of Mörigen, Estavayer and Auvernier whose names are inscribed, within a wreath of bullrushes, on the back. The medal, 37 mm. in diameter (or should we say, to be in period, No. 11 on Mionnet's scale) is by the artist Édouard Durussel (1842–88), a Swiss who worked at the Berlin mint and at Paris before settling, in 1869, at Berne. He died at Préfargier, which overlooks the most famous of all the lake-dwellings, that of La Tène on Lake Neuchâtel. L. Forrer's *Biographical Dictionary of Medallists* (London, 1902–30), I, 679–82, kindly consulted for me by my colleague in the British Museum, Dr John Kent, places the lake-dwellings medal in the period 1880–6.

For us, the Swiss lake-dwellings are ancient laurels; they gave us the cultural names of Robenhausen, Cortaillod, and La Tène, but we have perhaps forgotten the immense impact of the recovery of such vast quantities of ancient relics—particularly those of normally perishable organic materials—which to this day are scattered throughout the museums of Europe and all too seldom displayed. The medal recalls this impact for us.

The exploration of the lake-dwellings was in full swing a century ago. The first, at Ober Meilen, was discovered in 1829, but was more fully exposed in the cold, dry winter of 1853–4, when the waters of the lake were so much reduced that farmers began to reclaim some of the marginal land. By the date of the book mentioned below, over 160 lake-dwellings had been identified on 22 of the Swiss Lakes. La Tène, its name the local fishermen's word for 'shallows' and derived perhaps ultimately from the Latin *tenuis*, was found in 1858 and continued to yield its treasures down to 1917. The piles or stone foundations of the settlements could be discerned in shallow water, and so could many of the relics which were retrieved by simply-contrived grabs or dredges. One instrument used consisted of a pair of pincers, of which one arm was affixed to a long pole, and the other worked by a string; another, a toothed rake or scraper, was dragged through the

mud by a pole, and had two other poles hinged to it near the head, so that greater direct pressure could be exerted. By this means, trenches were dug in the *Kulturschicht* or 'relic-bed'.

The first comprehensive account of the lake-dwellings in English appeared in 1866, and has an especial interest for us in Wales. Entitled *The Lake Dwellings of Switzerland and other parts of Europe*, it is a translation and re-arrangement (under the author's guidance) of Dr Ferdinand Keller's reports to the Antiquarian Association of Zürich, by John Edward Lee. J. E. Lee, F.S.A., lived at the Priory, Caerleon, and was one of those persons principally concerned in the establishment of a Museum at Caerleon (now a branch of the National Museum of Wales) where inscriptions and other remains from the Roman legionary fortress might be preserved. In 1862 he produced his *Isca Silurum*, which is the first general account of these finds. As a final coincidence, I may be permitted to add that it was in the grounds of his house at Caerleon that I conducted, last October, the most recent in the series of rescue-excavations on behalf of the Ministry of Works.

GEORGE C. BOON

A SCHOOL OF AFRICAN ARCHAEOLOGY

Since 1957, the Rhodes-Livingstone Museum has run an annual School of Archaeology for a fortnight annually for the benefit of amateur archaeologists in South Africa.

The object of the School is to provide a grounding in the techniques and cultures of African pre- and protohistory, by means of lectures and practical demonstrations, including three days' excavation in the field. Museum techniques are also covered.

While the majority of lectures are given by the staff of the Rhodes-Livingstone Museum, particular subjects such as physical anthropology, rock art, geology and the Iron Age of Southern Rhodesia are covered by visiting experts from all over Southern Africa.

The course is an invaluable grounding for keen amateurs, and already one National Monuments Commission specifies attendance as a condition for granting an excavation permit.

The 1961 School will be run from 25 August to 8 September, and visiting lecturers will include Professor G. Bond, Professor L. H. Wells, Mr C. K. Cooke, and Mr A. Whitty. Three days will be spent excavating on an Iron Age site at Kalomo, 80 miles north of Livingstone. The course will be under the direction of the Keeper of Prehistory at the Museum, and applications to attend should be addressed to The School of Archaeology, c/o Rhodes-Livingstone Museum, P.O. Box 124, Livingstone, N. Rhodesia.

STONE TIPI RINGS

The High Plains of western North America were formerly dotted with circles of boulders, referred to as 'tipi rings' by local inhabitants, but considered by several contemporary archaeologists to represent aboriginal constructions of problematical, and presumably esoteric, function. In 1952, the writer, then Curator of the Museum of the Plains Indian at Browning, Montana, began a four-year study of the boulder circles. The study combined an intensive archaeological survey of the Blackfeet Indian Reservation, a 2000-square-mile area in north-central Montana, with ethnographic interviews of elderly informants from the Blackfoot and neighbouring tribes. The results of this study have been published in the United States,[1] but their possible application to Old World archaeology has prompted the following review for a British audience.

[1] Anthropological Paper 62, Bureau of American Ethnology *Bulletin* 173 (1960); also, in condensed form, in *American Anthropologist*, vol. 60, no. 5 (October 1958), 861–73.

Tipi rings on the Blackfeet Reservation were approximately-regular circles, between 7 and 30 ft. in diameter, constructed of one or two (concentric) rings of boulders, laid on the ground surface and never higher than a single course. Neither rock-lined hearths nor distinct doorways were usually present, although occasionally observed. Prepared or hard-packed floors were not indicated in the rings excavated. Artifacts and other cultural debris were rare. These absences of evidence of domestic use had been assumed to argue for an esoteric function of the rings, yet the quantity of rings recorded on the Reservation (one of the few areas on the Plains still largely undisturbed by the plough) suggested that they must have been frequently built. There must be literally thousands of rings on the Reservation; the writer mapped well over 1000, in 210 sites, all visible on the surface. Only 63 sites had only one ring. Fifty sites had three or more rings, and of these 34 sites exhibited a patterned placement of the rings.

The Indian informants all agreed that the circles of boulders had been used to weight down the covers of their forefathers' skin lodges. Several informants identified tipi rings for me in the field, and two old men could take me to the last rings used by their own fathers and by other families known in their youth. Bull Head, a North Piegan (of the Blackfoot confederacy) then 84 years old, explained that his father's ring 'is like a keepsake to me, and I am protecting it. . . . There are other rings belonging to my father, but I am just protecting one.' Bull Head told me that tipi rings are called, in Blackfoot, *iskiman*, 'something to hold down the lodge'. Mrs Duck Chief, a 92-year-old North Blackfoot, specified that rocks could generally be associated with skin lodges: 'People with hide tipis used them, when the tipis were used often, for every day. The hides stretched more than canvas, so the rocks didn't roll off as they would from tight canvas.'

In 1956 the writer studied the large camp erected at Browning for the Blackfeet Tribe's annual North American Indian Days Celebration. This camp contained 39 canvas tipis, pegged down, but containing, in several cases, a looser canvas liner (hung from the tipi-frame poles about 5 ft. from the ground) with the lower edge weighted by boulders. Mae Williamson, a middle-aged South Piegan noted for the fine display tipis she erects at the Celebrations, said that 'the rocks were used to hold down and push out the tipi liner similar to their use in the old days. Quite a few of the other campers were using them, too, but most of them were too lazy to gather rocks, since they were not easily found at the Browning campground.' Measurement of the liner rings before and after the removal of the liner proved that very little displacement of the rocks is occasioned by the pulling up of the liner.

After the four-day Celebration, the campground was marked by rings of untrampled grass, the former peripheries of the tipis. The placing of robes (bedding) against the inner wall of the tent or liner had protected this area from all but superficial destruction of the grass. There was a ring of lightly-trodden earth around the middle of the tipi 'floor', but the most heavily packed earth in the camp lay *outside* the tipis, for this is where, now as aboriginally, most daily activities had taken place. Imperishable cultural remains were as scarce in the Browning campground as they are in archaeological tipi-ring sites.

A series of photographs[2] made of the tipi of Mrs Mary Ground, a South Piegan, convincingly demonstrated the close homology between the liner-weight rocks of 1956 and the archaeological rings. The first shots showed Mrs Ground, with her home-made stove and piles of household equipment, standing in the circle of rocks that had rolled off as her tipi was taken down. Then, when Mrs Ground had packed her goods and set out for her permanent home, only the circle of rocks remained on the prairie, with a small heap of blowing ashes the sole evidence of human occupation.

[2] Published in *op. cit.*

Tipi rings are not the only stone constructions known on the High Plains. There are also 'medicine wheels',[3] cairns with radiating lines of rocks that mark the last resting place of war chieftains; boulder effigies[4] that commemorate notable incidents in tribal histories; and fasting shelters, U-shaped enclosures 2 or 3 ft. high and just big enough for a man to sit in, built by youths keeping vigil in expectation of a vision of a guardian spirit. None of these classes of stone constructions is at all frequent. It should also be mentioned that tipis were weighted down by logs and brush in wooded sections, and by cut sods[5] in unglaciated, boulderless regions. Tent rings that are functionally equivalent to Plains tipi rings may be of more elaborate construction (cf. Piggott's study of the tent stances left by nomadic Himalayan traders).[6] Nevertheless, where boulders of moderate size are readily available, archaeologists excavating in regions where nomadism may be assumed (e.g. steppes, Palaeolithic) should be alert for circular rings that may be the only relics of ancient skin dwellings.

THOMAS F. KEHOE

[3] Kehoe, *Journal of the Washington Academy of Science*, vol. 44 (1954), 133–7; Dempsey, *Journal of the Washington Academy of Science*, vol. 46 (1956), 177–82.

[4] Kehoe and Kehoe, *Journal of American Folklore*, vol. 72, no. 284 (1959), 115–27.

[5] Noted by Maximilian (pp. 104, 199), in R. G. Thwaites, *Early Western Travels*, vol. 23, pt. 1 (1906). Cleveland.

[6] *Man*, vol. 44 (1944), 150–52.

NEW RADIOCARBON DATES FROM FRANCE

A year ago we published (ANTIQUITY, 1960, 147)[1] *a first list of C14 dates from France under the signatures of Professor J. Coursaget, Dr P. R. Giot, and M. J. Le Run. These dates were determined at the* Laboratoire du Radiocarbone *at Gif-sur-Yvette. We are happy to publish here some more French radio-carbon dates, by the courtesy of the same three authors. These dates relate mainly to the later prehistory of France; the Gif Laboratory has also determined many Pleistocene dates and many concerning French excavations in Africa, America and Asia. We hope to be privileged to publish these in due course. Here we are concerned only with the later prehistory of metropolitan France.*

The *Laboratoire du Radiocarbone*, Gif-sur-Yvette, has recently dated another series of Neolithic and Bronze Age sites from different parts of France, some of which are most interesting.

In Northern France, the strata B of the habitation site of the Montagne de Lumbres (Pas-de-Calais), excavated by Dom R. Prévost,[2] with sherds of so-called '*chasséen*' pottery, perhaps with Michelsberg influences, has been dated twice: 2410 ± 125 and 2610 ± 125 B.C., giving a mean date of 2510 B.C.

In Central Western France, the camp of Biard, Segonzac (Charente), excavated by Cl. Burnez,[3] has given charcoal from the bottom of the ditch, with Peu-Richard pottery, dated 2391 ± 137 B.C.

In South-Eastern France, the cave of Trou-Arnaud, Volvent, Saint-Nazaire-le-Désert (Drôme), which gave a rich series of pots of the so-called '*chasséen*' ware, published by

[1] For the administrative aspect of the activity of the *Laboratoire du Radiocarbone*, *cf.*: *Centre National de la Recherche Scientifique, Rapport d'Activité*, 1958–9, 98–9; *Rapport d'Activité*, 1959–60, 97–100.

About radio-carbon dates of Brittany, see P. R. Giot, *Annales de Bretagne*, LXVII, 1960, 33–44; LXVIII, 1961, in the press.

[2] R. Prévost, *Bulletin de la Société Préhistorique Française*, LV, 1958, 162–5; LVI, 1959, 161–2; L. Aufrère, *Gallia*, XIV, 1956, 169–73; *Gallia-Préhistoire*, I, 1958, 95–6.

[3] Cl. Burnez, *Bulletin de la Société Préhistorique Francaise*, LIV, 1957, 535–549.

A. Blanc,[4] contained burnt corn which has been dated 2179 ± 135 B.C. This dating is somewhat younger than expected on considering the archaeological context.

In Brittany, the long barrow of La-Croix-Saint-Pierre, Grée-de-Cojoux, Saint-Just (Ille-et-Vilaine), excavated by P. R. Giot,[5] with Middle Neolithic pottery, has been dated: 2310 ± 120 B.C. On the other hand, burnt acorns picked up by M. Jacq in the central hearths of the Kerlescan long barrow, Carnac (Morbihan), excavated during the war by German archaeologists,[6] have given a date indicating some contamination or disturbance: 402 ± 110 B.C. In the same way, charcoal and wood from the angled passage grave of Poulguen, Penmarch (Finistère), excavated successively by A. Du Châtellier, a century ago, and A. Martin and P. Du Châtellier at the beginning of this century,[7] have also indicated some late re-utilizations and disturbances in the monument: 1601 ± 120 B.C. in one chamber, 500 ± 105 A.D. in the other. This is the sort of thing one should expect very often in these huge collective tombs, in which it is very difficult to find remainders of the original material.

Near the Mediterranean, a small megalithic tomb, La Bouissière, Cabasse (Var), excavated by G. Bérard,[8] produced some charcoal and burnt resin, which have been dated twice: 2025 ± 130 and 2010 ± 130 B.C. Though found under the paving of the monument, these substances do not perhaps either of them come from the initial use of the tomb.

In Corsica, the strenuous and fruitful work of R. Grosjean has disclosed, amongst other revelations, a whole culture with monumental towers of dry-stone cyclopean construction, which he names the '*torre*' after the denomination of one of them.[9] The monument of Tappa, Porto-Vecchio (Corse) is older than expected: two samples of charcoal, from different places in it, have been dated; Tappa I: 2298 ± 110 B.C. and Tappa II: 1907 ± 130 B.C. On the other hand, the '*torre*' of the celebrated site of Filitosa, Sollacaro (Corse), was only dated to: 1193 ± 105 B.C. These dates are to be compared with those obtained for the early '*nuraghi*' such as Barumini, Cagliari (Sardinia): 1460 ± 200 B.C. (Copenhagen K–151).

In Western France, the settlement site of La Viaube, Jaunay-Clan (Vienne), studied by M. Taillet,[10] from the Middle Bronze Age, has been dated: 1181 ± 135 B.C.

And in Brittany, the charcoal from the hole where a vast hoard of 800 socketed axes of the Late Bronze Age was discovered recently at Saint-Bugan, Loudéac (Côtes-du-Nord),[11] has given: 559 ± 130 B.C., which proves that these huge Armorican hoards are in reality of the Early Iron Age.

After having produced these dates, the equipment of the *Laboratoire du Radiocarbone* is being renewed and installed in a new building, specially designed. It is hoped to produce many more such interesting dates in the near future, and also to be able to begin to tackle French Palaeolithic sites.

J. Coursaget, P. R. Giot and J. Le Run

[4] A. Blanc, *Bulletin de la Société Préhistorique Française*, LII, 1955, 553–6; LIV, 1957, 118; *Cahiers Rhodaniens*, III, 1956, 22–32.

[5] P. R. Giot, *Gallia*, XII, 1954, 401–3; P. R. Giot and J. L'Helgouach, *Annales de Bretagne*, LXII, 1955, 282–92.

[6] (M. Jacq), *Gallia*, V, 1947, 162–7.

[7] A. Martin, *Bulletin de la Société Archéologique du Finistère*, XXIX, 1902, 23–33.

[8] G. Bérard, *Bulletin de la Société Préhistorique Française*, LI, 1954, 281–88 ; LII, 1955, 666–7.

[9] R. Grosjean, *Gallia-Préhistoire*, I, 1958, 1–38 ; *Bulletin de la Société Préhistorique Française*, LVII, 1960, 296–302 ; *Revue Archéologique*, II, 1959, 15–40.

[10] M. Taillet, *Congrès Préhistorique de France*, XIV (Strasbourg-Metz), 1953 (1955), 607–25.

[11] P. R. Giot, *Bulletin de la Société Préhistorique Française*, LVI, 1959, 169–71; *Gallia-Préhistoire*, III, 1960, in the press.

PLATES XVIII and XIX

THE WINGED OBJECTS IN THE LLYN FAWR HOARD

In his review[1] of M.-E. Mariën's *Trouvailles du Champ d'Urnes et des Tombelles Hallstattiennes de Court-Saint-Etienne* Dr J. M. Coles has rightly stressed the close parallels between various pieces in the Llyn Fawr (Glam.) hoard and objects from Tombelle A at Court-Saint-Etienne. The Belgian evidence is important not only because it provides the immediate continental background to the British hoard, but also because it has been possible for M. Mariën to attempt a reinterpretation of the so-called winged chapes from Llyn Fawr. In a preliminary discussion,[2] M. Mariën had suggested that these objects were intended to link the belt and shoulder-straps of a sword harness. His current view,[3] however, is that they are the cheek-pieces for a horse-bit, an explanation which Dr Coles finds 'not entirely convincing'. The purpose of this note is to show, with the aid of specially prepared photographs and drawings,[4] that it is indeed most probable that these bronzes are cheek-pieces.

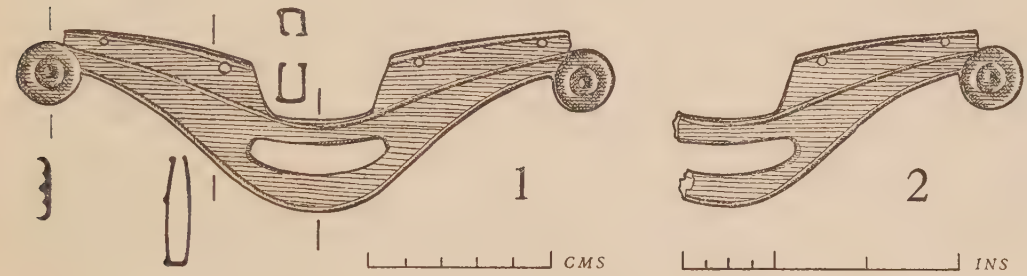


Fig. 1. Bronze cheek-pieces from the Llyn Fawr (Glam.) hoard.

The so-called winged chapes from Llyn Fawr comprise two pieces—No. 1 complete, and No. 2 lacking almost a half—which are so closely similar as to suggest that they should be thought of as a functional pair, rather than as two unconnected objects. They are hollow castings in thin bronze, finished with great delicacy on the side (here called the upper side) which bears raised ridges and ornamental discs. On the underside, however, the casting-seams have been only partly filed down, though much of that side has been rubbed smooth in use. Clearly only the upper side was made to be seen. No cross-sections have hitherto been published, yet as FIG. 1 shows they are of great importance; for while the top edge of each wing (A-A) is open to take straps (as the pairs of rivet-holes show), the top (but not the sides) of the central U-shaped depression (B) is closed. This at once precludes interpretation of the objects as winged chapes since the top edge of a chape must have a central opening to accommodate the tip of the scabbard, whereas the wings should be closed or even cast solid.

[1] ANTIQUITY, XXXIV (1960), 314–6.

[2] Mariën, M.-E. Note sur quelques objets hallstattiens de Court-Saint-Etienne, *Bull. Mus. Roy. d'Art et d'Histoire*, 18 (1946), 16–28.

[3] *Trouvailles . . . de Court-Saint-Etienne*, pp. 32–6; p. 237 with fig. 46.

[4] The photographs are by Mr Eric Broadbent, and the drawings were prepared by Mr Colin Burgess. I am grateful to Dr H. N. Savory for granting me every facility to study these objects in the National Museum of Wales.

Interpretation as part of a sword harness remains possible, with the wings receiving the shoulder-straps, and the central slot (C) taking a loop or hook to the belt. To this, however, there are two immediate objections. Firstly, the angle of the wings and disposition of the rivets on the complete piece from Llyn Fawr suggest that it accommodated straps whose centre line converged on a point about 9 in. distant. Shoulder straps, on the other hand, might be expected to diverge from the small of the back to the shoulders. (The evidence of the Court-Saint-Etienne pieces is ambiguous, partly because of their damaged state; they may indeed seem better suited for diverging straps.) Secondly, the pieces from Llyn Fawr appear to be a pair, and the same may be said, allowing for minor differences, of those from Tombelle A. This in itself seems to demand an interpretation which accounts for their use in pairs. In each case they are associated with only one sword.

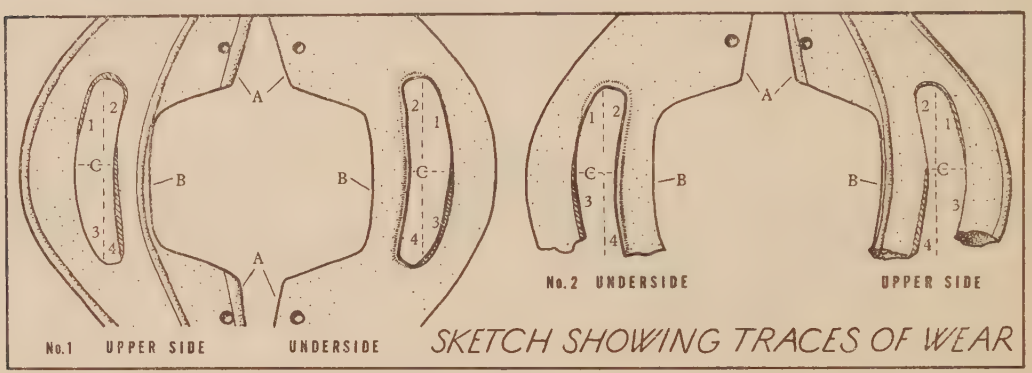


Fig. 2.

It remains to consider the clearly defined traces of wear through use (shown diagrammatically in FIG. 2 and also in PLATE XVIII) which provide fundamental evidence for interpretation.[5] Apart from the general wear of the underside, these traces are localized around the central slot C. Had the objects been used for a sword harness, with this slot taking a loop or hook from the waist-belt, it might have been expected that wear would have occurred along the edges C1 and C3, being especially concentrated near the junction of these edges, dying out symmetrically in either direction, and being equally pronounced on both upper and under sides. In fact there is no such wear. The most pronounced rubbing, which has produced a marked facet, is on the upper side, along the edge C4 with a slight extension into C2. This seems to have been caused by the friction of a metal bar or other solid object pulled hard back against the upper C4 edge. Such a bar would have passed diagonally through the slot; so on the underside, we find matching traces of wear along the C3 edge. Apart from this very pronounced faceting along the upper C4 edge, there are slighter traces of rubbing around the junction of edges C1 and 2 on the upper side, matched apparently by some wearing of the casting-ridge along the C1 (and to a lesser extent the C2) edge on the underside. This again appears to have been caused by a solid bar passing through slot C, but with less pressure or tension applied than in the first case.

To sum up, the wear on these two winged objects suggests very strongly that a metal

[5] Bökönyi, S., Reconstruction des mors en bois de cerf et en os, *Act. Arch. Acad. Sci. Hungaricae*, III (1953), 113–21.

bar had passed diagonally through the central slot; that sometimes it had been pulled back, with great force, against the lower (i.e. C4) margin of the slot; but that at other times it had lain more loosely at the upper (i.e. C1–2) end of the slot. It will be apparent that this could well describe the positions of a horse-bit, firstly when the horse was reined back and secondly with the reins slack, when the cheek-pieces would slide down the bit to the limit of slot C.[6] Two confirmatory observations may be added. Firstly, a careful study of FIG. 2 will make it clear that the wear on pieces 1 and 2 can only be matched if they are placed as an opposed pair or back-to-back, as of course cheek pieces would be. Secondly, pairs of antler cheek pieces from the Swiss Lakes[7] (which are themselves probable prototypes of the simple bronze cheek pieces from Tomb 2 at Lengenfeld from which M. Mariën would derive the Court-Saint-Etienne and Llyn Fawr group), show clear traces of wear through use back-to-back.

In view of this strong and consistent evidence in favour of M. Mariën's latest interpretation it remains to re-examine Dr Coles's objection to it. He has said that 'the strain imposed on the leather attachments via the neck harness surely would necessitate a firmer method of fastening leather to such a piece'. It is not entirely clear what Dr Coles means by neck-harness; the Llyn Fawr cheek-pieces, as the angle of the wings shows, would be joined to the headstall by a Y-strap. When the horse was reined-in, the bit and cheek pieces would be pulled back, and there would then be no strain on the attachments. When on the other hand the reins were slack the only strain on the Y-strap would be that of the weight of the cheek pieces themselves—rather less than 4 oz. the pair. In any case, it must not be thought that the cheek pieces were held merely by two (or in an example from Tombelle A one) slender rivets. These thin bronze castings are so flexible that as the rivets were hammered down the two sides of the strap slots (A–A) would be pulled together. The straps would then be gripped not only by the rivets, but by two metal edges more than 2 in. long. Once these points are understood, it seems that Dr Coles's objection must fail. Conversely, in view of the evidence adduced here, it may be suggested that M. Mariën's interpretation should stand, with all its implications for Hallstatt horsemen in Britain around 600 B.C.

LESLIE ALCOCK

[6] In the absence of bits at Llyn Fawr and Court-Saint-Etienne, the detailed mechanics of the cheek pieces are slightly uncertain. Evidence from Lengenfeld suggests that a two-link bit of bronze would be appropriate. For greater certainty in the interpretation of these pieces it would be necessary to experiment with facsimiles on a horse of the right size, e.g. something like a Shetland pony, *not* the breed used by M. Mariën for his reconstructions. I am grateful to Professor R. J. C. Atkinson for his observations on this.

[7] e.g. from Mörigen in the British Museum, W. G. 1384, 1385, seen through the kindness of Mr J. Brailsford.

PLATE XX (*b* and *c*)

THE FOLLY IN COBHAM PARK

To the N.E. of Cobham Hall lies a pleasure garden known as 'Lady Darnley's Garden'.[1] This was part of the improvements and landscaping carried out by Humphrey Repton in the 1790s. One feature of this garden, now much overgrown, is a large number of sarsen stones. Single stones are dotted about with no apparent intention to form a plan; an ice-house in the hillside has sarsen stones forming the jambs and lintel of its entrance, whilst standing on the edge of a spur overlooking the garden is a 'dolmen' of two vertical

[1] *Guide to Cobham Hall and Park.* (Ralph Arnold.)

stones and a capstone (PLATE XX(*b*)). The ' dolmen ' is about 5 ft. high and beneath lies the shell of a giant clam.

The only definite date that can be given to any of these is to the ice-house; even then there is no mention of the sarsen stones. In the Estate Steward's Account Book for 1792[2] there is an entry that records that between 6 November and 16 December £20 19*s.* 2*d.* was paid to bricklayers for work done on the ice-house.

Local legend states that these stones once formed a genuine megalithic monument at a place called Battle Street, in the village of Cobham about a mile distant to the West. The name is suggestive and the spot was marked on the older O/S 6-in. maps as 'Druid's Temple'. Mr F. C. Lucas, speaking to a meeting of the British Archaeological Association in Rochester[3] in 1854 appears to have placed implicit trust in the memory of an old man, one John Gill. Twelve years previously Gill had told him that in about 1770 a group of large stones at this spot had been demolished at the order of the late Lord Darnley and they had been used to make a rockery at the Hall. In 1842 the late Lord Darnley would have been the fourth Earl who was aged three in 1770. That there were stones in the Battle Street area is confirmed by entries in the diary of Richard Hayes, a farmer who farmed this land and lived at near-by 'Owletts' in the late 18th century. Three entries quoted by Payne,[4] two in March 1770 and one in February 1773, indicate that stones were collected in the Battle Street area and set up along the roadsides near to 'Owletts'. Many of these stones remain today at Rookery[5] Corner, the cross-roads at which 'Owletts' stands.

Sarsen stones are common in the Cobham area; one is built into the fabric of the church, whilst there is another at the West end. About three miles to the south of Cobham at Harvel there is a group of stones, the Cockadamshaw stones, which are known to have been brought to this spot from the surrounding fields by farmers. The stones in the pleasure garden in Cobham Park might conceivably be the remains of a cromlech that once stood near Battle Street, or they might be natural stones that intruded upon Repton and his workman whilst landscaping the Park. Here were romantic objects to stand amid the Classical Temple (PLATE XX(*c*)), the pedestal urns and the thrones of the Gods.

H. A. JAMES

[2] Cobham Estate Papers. (Kent County Record Office.)
[3] *J. Arch. Assn.*, IX 1854, 427.
[4] *Collectanea Cantiana.* Payne. p. 153.
[5] Possibly a corruption of 'Rockery'.

WASH COAST-LINE LEVELS SINCE ROMAN TIMES

As Mr Green, in his stimulating article on East Anglian coast-line levels in the last number has referred to work by my husband and myself on settlement history round the Wash, you will perhaps allow me to summarize briefly the general picture of changing drainage conditions and, by implication, changing levels,[1] which has emerged from this work,

[1] Mr Green has rightly stressed the difficulty of equating drainage with level changes for the Wash area. Too many unknown factors are involved. C. G. mentions compression, which must certainly be a big factor near the Wash, where 30–40 ft. of soft clays, silts, and peats overlie the 'solid'; difference, and possibly changing difference, between tidal range and mean levels in the Wash and on the open coast; and I would add our ignorance of actual and effective distance of settlements from the contemporary coast, of degrees of silting in estuaries and foreshore at standstill or slow submergence, and the extent to which these outfall conditions can affect the backing-up of drainage water in fresh-water, peat fen, areas.

particularly as it is not altogether the same as that that he draws. It is only when we have each regional story in full that we can compare, contrast, and hope to explain.[2]

The work of the Fenland Research Committee in the thirties[3] has made familiar the sequence of *early* deposits which extend from the Wash deep into the southern portion of the Fenland basin—a lower peat, or peats; the 'buttery clay' of the Late Neolithic transgression; and an upper, mainly Bronze Age, peat. But the *later* extensive marine silts and clays which in part overlie, in part equate with, some parts of this peripheral sequence, do not extend over the whole basin southwards towards Cambridge and the chances of investigation. They are much harder to tackle with a hand borer. Consequently, even a recent summary, like Smith's comparison of the North Lincolnshire with the Wash sequence[4], finds an apparent contrast, which is there only because he has left out of account the comparable late deposits, which reach their fullest development only round the Wash margin. It was only during a brief part of their long, multi-phase development, that these post-Bronze Age Wash 'silts' extended along tidal levees back deep into the peat areas, and so only this part of their history has been related in detail to the earlier sequence.[5]

A series of concentric zones of settlement, the youngest nearest the Wash, the oldest furthest from it, lie on the surface of the marine alluvium; post-medieval settlements nearest the sea; an arc of medieval townships behind their protective bank; and, most remote from the sea, nearest the peat fen, a broad zone showing surface traces of Romano–British occupation,[6] visible from the air as the crop- and soil-marks of the ploughed-out ditches of droves, fields, and groups of small enclosures, the latter coinciding with groups of scatters of domestic debris exposed to ground survey by the plough.[7] Though it is convenient to refer to the whole area as Wash 'silts', in the broad geological sense, the soils range from homogenous silts at about 10 to 12 ft. O.D. and higher, in the zone of medieval settlement; to a very heterogenous mixture of raised silty levees and spreads at about 7 to 10 ft. O.D. with intervening clays dropping to 4 ft. O.D., in the zone of R.B. settlement.[8]

Commercial bores,[9] and those made by Mr J. N. Jennings,[10] from this R.B. occupation

[2] The work of Dutch and German scholars has taught us to expect the same level changes to have quite different consequences along different stretches of the coast, depending on differences in initial topography and the subsequent history of deposition. For the Netherlands see 1954, I. J. Bennema, 'Bodem– en Zeespiegelbewegingen in het Nederlandse Kustgebied', *Boor en Spade*, VII, for general and regional summaries and detailed references; and for the adjacent North German coastline, 1950, Carl Schott, *Die Westkuste Schleswig-Holsteins*, Schriften der Geographischen Instituts der Universitat Kiel, Band XIII, Heft 4.

[3] C. W. Phillips, 1951, 'The Fenland Research Committee', in *Aspects of Archaeology*, presented to O. G. S. Crawford, 258–73, with full bibliography. 1940, H. Godwin, 'Studies in the post-Glacial history of British vegetation. IV. Post-glacial changes of relative land- and sea-level in the English Fenland', *Phil. Trans. roy. Soc.* London, Series B, No. 230, 285–303.

[4] 1958, A. Smith, 'Post-Glacial Deposits in South Yorkshire and North Lincolnshire', *New Phytologist*, 57, 19–49, especially summary, 44–7.

[5] 1933, G. Clark, 'Report on an Early Bronze Age site in the South-Eastern Fens', *Ant. J.*, XIII, 266–96. 1950, J. N. Jennings, 'The Origin of the Fenland Meres', *Geological Magazine*, LXXXVII, 217–25.

[6] Extent indicated in 1953, J. K. St Joseph, 'Air Reconnaissance of Southern Britain', *JRS*, XLIII, 95–6, and pl. VIII; 1956, Ordnance Survey Map of Roman Britain, third edition, revised reprint; and 1960, S. J. H. in *Linc. Arch. and Arch. Soc. R. and P.*, n. 8, Map A, p. 36.

[7] For an example of the detailed layout which can be obtained by combining the plotting of R.A.F. vertical photographs and ground survey, see S. J. H., *op. cit.*, Map C, and general map of the author's South Lincolnshire survey, Map B.

[8] For soils, see G. I. Smith, 1937, *The Land of Britain*, The Report of the Land Utilization Survey of Britain, Part 69, Lincolnshire (Parts of Holland).

[9] Information from River Welland Catchment Board and Geological Survey.

[10] Field work 1951–2.

surface, show it to be separated by a deposit of up to 14 ft. of clays and silts, thinning as it is traced away from the sea, from a peat which pollen analysis by Jennings[11] has shown to correspond to the *upper* peat of the southern fens, though it lies at a slightly lower level (minus 7 to 8 ft. O.D.) because of the compression of the further 20 ft. of clays and (two) peats beneath it.

These very thick and extensive post-Bronze Age, pre-Romano–British inorganic deposits, reaching right to the fen-edge in Kesteven, but not extending into the southern and western bays of the fens (except as, e.g., the clay flanges of the Little Ouse roddon) must mark a considerable submergence, roughly corresponding with that which, in the Yarmouth area, Mr Green believes to have reached its maximum in the 1st century B.C. For the Wash area an earlier and more marked recovery seems indicated. The upper few feet of the pre-Roman deposits become more silty, and on them it was possible for numerous small-scale native-type settlements to be established by the last quarter of the 1st century A.D., their irregular enclosure layout closely related to the silty levees of a network of minor tidal channels, their economy based on salt evaporation and sheep pasturage on the now dry salt flats. Early in the 2nd century a more grandiose scheme could be launched, showing the hand of the Roman engineer in its straight roads and canals, and even rectilinear minor droves, enclosures and field grids, though this would become modified later. Settlement density was at an optimum in the late 2nd and early 3rd centuries, with the emphasis on cattle, and surface traces of over 300 settlement nuclei have been plotted. There is, *pace* Mr Green, *no evidence* of any seaward protection. (The 'Roman' bank is a Stukeley myth.)

The deterioration of drainage conditions, which followed in later Roman times and after, is neither so 'slight' nor so 'secondary' as Mr Green would suggest. The major artificial Roman channels, like the natural watercourses into which they run, are flanked on either side by high levees of estuarine silt, and these in turn are delimited on either side by parallel soak-drains, closely linked with minor drainage channels and the rest of the Romano-British field layout. These levees are thus shown to have formed, at least in main part, *during* the Romano-British period, and for the late R.B. period we must evidently envisage a system of sluices, between the soak-dikes, receiving field drainage, and the major tidal channels whose waters might lie at a higher level in some conditions of the tide. A study of the changing distribution of R.B. settlement (based on the ground survey of 100 South Lincolnshire settlement nuclei by the writer, and pottery dating by Mr B. R. Hartley) corroborates the picture of increasing drainage difficulties. There was a progressive shift of the zone of greatest population density, mainly in the 3rd century, from the smaller, more fenward settlements, liable to fresh-water flooding,[12] towards larger nuclei (villages?) on the higher silts further seaward, though occupation was able to continue inland on the raised levees of major channels. On the fenward fringes, indeed, some large areas of clear detailed layout visible from the air, but corresponding to only the most exiguous traces of occupation on the ground, suggest that the official scheme over-reached itself from the first. Very few Lincolnshire silt settlements survived into the late 4th century. Detailed plotting of the air photographs shows flood channels breaking out from the artificial, as well as the natural, layout, and the blanketing of whole areas under flood silts, blending seawards into the relatively homogenous zone of silts, which

[11] *In litt.* Presence of Fagus, and high percentage Tilia, suggests late VII or VII–VIII. This equates with the *Lower* Peat at Ingoldmells. See 1958, A. Smith, *op. cit.*, p. 44.

[12] I cannot speak for Mr Bromwich's *fen-edge* sites, but for the 'silts' an extensive programme of levelling has not made it possible to estimate feet of flood water.

overlapped[13] the seaward fringe of the R.B. settlement zone, and on which the early medieval settlements were established.

The late Roman deterioration, and the extensive post-Roman pre-medieval marine deposits (which may well include the Scrobicularia clay at Ingoldmells, and the top clay at St German's) would correspond with a period of rapid *submergence*, not emergence, by A.D. 300, which has no place in Mr Green's story, though it agrees well with the work of scholars on the other side of the North Sea.[14]

From this point, both the Yarmouth and the Wash areas attest an early medieval emergence, which made resettlement possible by the 7th to 8th centuries, an optimum in the 9th to 11th centuries, and a further deterioration which really made itself felt by the late 13th, and accelerated in the 14th and 15th centuries.[15]

SYLVIA HALLAM

[13] The digging of the Coronation Channel east of Spalding revealed a 1st to late 3rd-century occupation at about 5 ft. O.D., under 5 ft. of estuarine silt. Other odd finds confirm that the medieval townships, and, of course, their sea banks, lie on post-Roman silts.

[14] In the Netherlands extensive unprotected settlement on the flat in Zeeland, the Rhine-Maas area (e.g. the Bommelerwaard), South Holland (e.g. Westland) and North Holland, was abandoned by A.D. 300 and covered by marine deposits; while in Friesland and the adjacent north German areas, where earlier marine sedimentation of the period 700–300 B.C. had built up to a higher level, some occupation was able to continue on raised 'terpen'. Resettlement of young marine deposits in the south became possible in the 7th to 8th centuries, diking was necessary about the 10th century, and the 14th to 16th centuries brought high storm surges and serious catastrophes. See note 2.

[15] H. E. Hallam, *The New Lands of Elloe*, A Study of Early Reclamation in Lincolnshire, Occasional Papers no. 6, University College of Leicester, 1954.

Mr Charles Green comments:

Mrs Hallam's commentary is most valuable. But I am not yet quite convinced that the late Roman submergence in the Fens cannot be explained by land-shrinkage due to compaction. Thus Godwin's graph of relative level-changes at St Germans does not show this intermediate oscillation and as this site lies only some two miles from the upland, it seems probable that there the compaction would tend to be less than that in the middle of the Fenland basin, where so much of Mrs Hallam's work has been done.

I also incline to the view that compaction would tend to be greatest close to the southern edge of the post-Bronze Age silt deposits, and this lowering of surface-level would slowly creep northward towards the Wash coast as the effects of time and the added weight of new silt became felt. This would seem to be marked by the seaward shift of settlements; but, as we both have said, we do not know and it is indeed difficult to devise methods of resolving this problem. An opportunity to examine closely various occupation-levels flanging out into the marsh in the Yarmouth district may yet show a similar oscillation there, though so far all the available evidence points the other way. I have never thought that Yarmouth district and Fenland oscillations should be precisely similar either in date or extent, but in the absence of contradictory evidence, evidence from the one may hint at the broad changes which took place in the other.

It was not only Yarmouth evidence which suggested that the maximum of the Iron Age submergence fell in the 1st century B.C. The Essex 'Red Hills' were certainly used in the early part of the Iron Age as the land submerged. This usage came to an end towards its close for, as R. A. Smith said, there was evidence of Romano-British occupation later, though not as salt-pans. Confirmation comes from a recent paper by F. T. Baker (1960), who described a section near Ingoldmells, Lincs. Here the earlier occupation (Iron Age 'A2') was separated from the overlying 1st-century A.D. 'Belgic' activity by a band of

marine deposition representing a short break only. Further north, A. Atkinson (1884) noted a corduroy causeway across the river Ancholme near Brigg at the level of the Late Bronze Age peat, dated by a bone bridle cheek-piece, apparently of Iron Age 'A' type. This was overlaid by estuarine clays capped by peat beds containing Romano-British pottery. So close a correspondence of date is perhaps rather surprising as a time-lag might have been expected as the oscillation moved northward.

I am aware that there is no direct evidence for a 'Roman Bank' of Roman date, but it is difficult to conceive the undertaking of such large engineering activities without some seaward protection from surges. That these surges are not recent in origin is of course well-attested by the entries in the Anglo-Saxon Chronicle (Peterborough MS) for A.D. 1014 and 1099. Moreover the unpublished evidence from Welney (from the late E. J. Rudsdale) looks uncommonly like the results of an unexpected surge at a much earlier date.

I should say that Mrs Hallam and I differ rather in emphasis than in fact. We need more evidence to determine the truth.

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INTERNATIONAL CONGRESS OF PREHISTORIC AND PROTOHISTORIC SCIENCES

The Sixth International Congress of Prehistoric and Protohistoric Sciences will be held in Rome from 29 August to 3 September, 1962. The meetings will be held in the buildings of the *Universita degli Studi, Cité Universitaire*, and there will be excursions before and after the Congress to various parts of Italy as well as to Sicily and Sardinia. Those wishing to enroll as members of the Congress should write to the Secretariat of the Congress, *Museo Preistorico Etnografico 'L. Pigorini', Via del Collegio Romano, Rome.* The President of this Congress is Professor M. Pallottino, the Vice-Presidents Professor Bernabò-Brea and Professor Paolo Graziosi, and the Secretary-General Professor Luigi Cardini.

COWA SURVEYS AND BIBLIOGRAPHIES

Attention has already been drawn to the existence of the Council for Old World Archaeology (ANTIQUITY, 1959, 60). Its chief purpose is to aid scholars to keep abreast of the latest activities and publications in the prehistory and archaeology of the Old World and this it does by publishing biennially a series of *COWA Surveys and Bibliographies* consisting of twenty-two area reports on the archaeology of the entire Old World from Palaeolithic to recent historical times. The first series was published in 1958 and 1959. The first four areas of Series II (British Isles, Northern Asia, Eastern Mediterranean, and Indonesia) are recently published and have just been distributed to subscribers. The whole effort to date will be reviewed in the next number of ANTIQUITY. Meanwhile we remind readers of these valuable *Surveys and Bibliographies*. The annual subscription price of 7 dollars 50 cents pays for eleven areas. Single areas may be ordered at one dollar a copy. Series I (1958–9) is still available (except for Western Asia, which is exhausted) at 4 dollars for each year. These publications can be ordered direct from the *Council for Old World Archaeology*, 11 *Divinity Avenue, Cambridge* 38, *Massachussetts, U.S.A.*

Reviews

THE JOURNAL OF ROMAN STUDIES, Vol. L (1960). Jubilee Volume. *pp.* xi + 293, 27 *plates*, 35 *figures*. £3 *to non-members of the Society for the Promotion of Roman Studies, obtainable from the Society*, 31–34 Gordon Square, London, W.C.1 *and through booksellers.*

This volume is not a mere symposium of learning; it is a monument. And if it is a monument to one man more than another, it is a monument to Francis Haverfield. Without him, it is safe to suppose that no Society for the Promotion of Roman Studies, whether under that ill-shaped name or any other, would have been founded in 1910. Haverfield has been dead this forty-two years, and I never had speech of him. But his powerful presence is still immanent in an appreciable province of Roman studies; as it was, I remember, on that August day of 1913 when his approaching visitation darkened the Wroxeter scene as by a kind of eclipse. Throughout the morning of that day Bushe-Fox, director of those celebrated excavations, flitted with unwonted apprehension from site to site, and at lunchtime sharply and suddenly banished us students as something not quite nice to have about the place. At least he must have us out of sight and earshot. In fact we ate a silent tea in Shrewsbury, 12 miles away, and returned timorously after dark. A tired Bushe-Fox was smiling quietly to himself; he had passed The Test, but there was still, it seemed to us, a whiff of brimstone in the air.

It was a part of his genius that Haverfield was well served. The volume opens with a gracious portrait of Margerie Taylor and a just and eloquent testament to her achievement and her person. She was Haverfield's skilled assistant, and perhaps only those of us who have had occasion to work through some of the infallible material which she prepared for his contributions to the Victoria County History can fully understand his indebtedness to her. Since 1920, when I first met her, her debtors have become legion. She has cloaked a tireless generosity under a veil of intolerance which has deceived only those who do not know her. In this dual quality she is, no doubt, in true line of succession to Haverfield himself. Of her single-minded devotion to the Society there is no manner of doubt; the distinguished list of Presidents whom she has controlled, and above all the remarkable line of fifty *Journals* for which she has been in great measure responsible, are her witness. Her services were aptly acknowledged in her own recent Presidency.

The passage of fifty years encourages reminiscence, and several of the contributions to this volume are in that vein. Miss Taylor herself recounts the origin and growth of the Society, and other writers deal authoritatively with half a century of Republican History, of the History of the Roman Empire, of Roman Religion, of Roman Britain, of Discoveries in and about the Roman Forum, and of Roman Inscriptions. There are also more than a dozen articles on a wide variety of more individual subjects, including a skilful reincarnation of Pelagius and an interesting and serious attempt to debunk the 'Ara Pacis'. I propose only to add a comment on Professor Ian Richmond's admirable survey of 'Roman Britain, 1910–1960', as a topic near alike to Haverfield and to Miss Taylor.

I have referred to the enduring quality of Haverfield's work on Roman Britain: in particular his chapters in the Victoria County History, his additions to the Corpus of Latin Inscriptions, and his slim volume on *The Romanization of Roman Britain*, which never much outgrew its original scope as a paper in the *Proceedings of the British Academy*

for 1906. When it be recalled that nearly all the 'scientific' exploration of Roman Britain has occurred since that date, and that Haverfield was admittedly never notably susceptible to the niceties of field-techniques (although Pitt-Rivers had completed his work by 1900), it is an astonishing thought that the broad outlines of our picture of Roman Britain remain very much as he left them. The truth is that Haverfield was concerned primarily with the landscape; he was little interested, or interested very secondarily, in botany. The wide expanses of woodland, rather than the species and incidence of the trees, were his theme. He was an artist; he thought and wrote as an artist. Almost any fool can be a sort of botanist. Almost any student with a *beta* mind can dig and record the minutiae of an individual site and sort out its potsherds. But there have been very few artists. Haverfield was one, Arthur Evans was another, and so were James Frazer and Flinders Petrie. The accumulative cleverness of a subsequent century of busy little men and women will not impair the intuitive and essential rightness of these artists.

What then is progress? What have we been adding to Haverfield, save fuller illustration? Let me quote again that revealing paragraph wherein Leonard Woolley describes (in 1953) his first experience of archaeological fieldwork in 1908. It was at the Roman military base-town beside Corbridge in Northumberland.

> 'I know only too well [he says] that the work there would have scandalized, and rightly scandalized, any British archaeologist of today. It was however typical of what was done forty-five years ago. . . . The committee naturally appealed to Professor Haverfield as the leading authority on Roman Britain and he, as he had intended to take a holiday on the Roman Wall, agreed to supervise the excavations. Somebody, of course, had to be put in charge of the work and . . . Haverfield arranged . . . that I should go to Corbridge. In point of fact, I had never so much as seen an excavation. I had never studied archaeological methods even from books (there were none at that time dealing with the subject). . . . I was very anxious to learn, and it was a disappointment to me that Haverfield only looked in at the excavations one day in the week and then was concerned only to know what had been found—I don't think that he ever criticized or corrected anything.'

Well, it is fair to say that we have advanced a little since that dreadful episode, and as assume that our successors will continue the process, with the reappraisal of such literary controls as have survived from antiquity and the advent of such new scientific controls to modern physics and chemistry—and indeed botany—may continue to provide. Already since 1908 we have, as we should, extended our detailed knowledge of little Roman Britain beyond that of any other Roman province. We have classified and reclassified our potsherds and our brooches and our table-legs. Each generation proudly and even scornfully amends the achievement of its predecessor, and must continue to do so if it is to justify its bread and its boot-leather. Many shall run to and fro, and knowledge shall be increased. We will gleefully move our data twenty-five or fifty years this way or that, and will add a few new ones. We will readjust, within increasingly narrow margins, the historical and archaeological successions to one another. The path of minor progress will continue to be strewn with Ph.Ds. and D.Phils. It is to be hoped that each generation will produce also an imaginative mind which can tidy up this messy path and prepare it for the next secular procession. So far we have been fortunate in this. After Haverfield we had the metaphysical Robin Collingwood, if only on a part-time basis. Today we have Ian Richmond, who is both technician *and* artist and will produce a splendid vision when some timely affliction confines him to a bath-chair. Meanwhile, we are slowly approaching a little more nearly to the *mind* of Roman Britain, for what that mind be worth—and all

human mentality is presumably worth something. We must not expect much of great consequence. Hadrian's Wall, for example, has long ceased to matter as a major historical problem. The annual assault upon it has dwindled to the level of a tactical exercise at gubernatorial or even C.R.E. level. These intimate little get-togethers are of tremendous and immediate value for training purposes. But for the major attainments of mankind the young scholar, thus trained, must now look to other periods, other lands. Haverfield's work was not merely a monument, it was a tombstone. And if that doesn't shake the local circulation of ANTIQUITY, I don't know what will.

MORTIMER WHEELER

MYRTOU-PIGHADES. A LATE BRONZE AGE SANCTUARY IN CYPRUS. *By* JOAN DU PLAT TAYLOR, F.S.A., *with contributions by others. Oxford: Ashmolean Museum*, 1957. *pp.* xl + 118, 18 *text figures*, 7 *plates*. 50*s*.

I have already reviewed this book in some detail in the *American Journal of Archaeology* (vol. 64, 1960, pp. 290 ff.) and anybody interested in the site can refer to that periodical. The best that can be said about this book is that the stratigraphical account is good enough for an expert to understand it; the worst that the chapters on the small finds are for the most part inadequate. An excavation has to be judged by the objects and the historical information which it produces and it must be confessed that the historical information gained at Pighades is not very great and has been thoroughly obscured in the publication. There were some magnificent finds, particularly of bronze rings and tripods, but these have been so inadequately published that they will have to be done again at some date in the future.

The major lesson to be learned from the excavations at Pighades is that Cypriot settlements are not worth excavating on a limited budget and that nothing except very large scale work will produce reasonable results. Another lesson from this report is that scientific enthusiasm can defeat its own ends; there has to be a reasonable compromise between the two extremes of archaeological charlatanism represented respectively by certain French and British schools of thought.

It must be confessed that a certain amount of the badness of this book is due to the failure of the University of Sydney to honour its moral obligations and to the even worse failure of Mr J. B. Hennessy to publish his parallel excavations at the nearby cemetery of Stephania; the first part of the manuscript of this only came into my hands late last year but we hope to start printing it later this year.

Miss Taylor deserves considerable praise for her promptitude in publication and what merit this book possesses is entirely due to her work.

JAMES R. STEWART

THE EGYPTIANS. *By* CYRIL ALDRED. *London: Thames and Hudson*, 1961. *pp.* 244, 82 *photographs*, 52 *text-figures*. 30*s*.

This recent addition to the series *Ancient Peoples and Places* is particularly welcome. No reliable book with the civilization of the Ancient Egyptians as its theme has been produced in Britain since Glanville edited *The Legacy of Egypt* in the early years of the late war. It is in fact true to say that British Egyptologists have nearly always left the general interpretation of their chosen culture to others whose books are distinguished more by enthusiasm and a superficial acquaintance with the well-known than by real knowledge and understanding. Mr Cyril Aldred, who is a specialist in Egyptian art and particularly in Egyptian sculpture, is also a practising craftsman who is especially well equipped to write an account of a culture that has left us such lavish material remains.

He begins his book with an account of the discovery of Ancient Egyptian civilization in modern times and the development of the science of Egyptology. The discovery of the Rosetta stone and the subsequent decipherment of hieroglyphics was the crucial event by which an esoteric mystery was transformed into a viable discipline. After surveying the development of the study both from the point of view of excavation and from that of epigraphy and philology he ends this section with an account of some of the tasks in excavation that need to be undertaken. Many important sites have scarcely been touched; no town-mound in the Delta has ever been examined properly. The shortage of competent excavators gives no hope that the list will be diminished in the near future.

The particular nature of Egyptian civilization was undoubtedly the outcome of geographical factors. In two chapters Aldred describes the country and its natural resources. The Nile is Egypt and here it is used as the highway by which the reader is conducted to the principal towns and sites, starting in the south and moving north. This is no dry catalogue of docketed place-names, but a revealing, lively tour, done in the best Hellenic tradition. In discussing copper, Aldred states that copper-ore was mined in Sinai and at sites in the Arabian desert. The evidence in support of ancient Egyptian copper-mining in Sinai is very slender. There is positive evidence of copper-mining on a large scale, but the most recent considered opinion is that 'there is not a scrap of evidence for believing that the miners were Egyptian' (Gardiner, Peet and Černý, *The Inscriptions of Sinai*, II, p. 7). On the other hand, one would accept more happily the view that the Egyptians came to Sinai only to mine turquoise if more turquoises were found used in actual jewellery. The large expeditions described in the inscriptions at Sinai seem grossly inflated if turquoise alone was to be the prize.

In five chapters Aldred sketches the outlines of Egyptian history, beginning the story in prehistoric times and ending it with the last efflorescence of native Egyptian culture in the Saite Period. In a book that sets out to give a general account of the Ancient Egyptians, it may seem a mistake in method to devote so much space to what appears to be straight history. When space is limited, however, as it is in the volumes in this series, some approach through an historical framework is essential when dealing with a culture that continued through three millennia. Aldred uses his historical account as a foundation on which to build a picture of Egyptian achievement in arts and crafts that it renewed with each successive period. The amount of information packed into these chapters is formidable and only occasionally can a statement be questioned.

It is suggested (p. 77) that the decline in the art of the potter from the beginning of the Early Dynastic Period may be due to the use of the wheel and the increasingly utilitarian aspect of the craft. The bulk of Egyptian pottery until at least the New Kingdom seems to be made without the use of the wheel and it can scarcely be more utilitarian than the extremely fine Predynastic wares.

The Step Pyramid is considered by Aldred to be a development of the great tomb-mound and its shape a result of the influence of Heliopolitan ideas (p. 85). The origin of the stepped structure, however, can probably be traced to the stepped inner mounds of 1st Dynasty tombs at Saqqara, and it is now generally thought that the change from stepped to true Pyramid was due to the development of Heliopolitan ideas (see Edwards, *The Pyramids of Egypt*, 2nd edition, pp. 24 and 236).

Aldred points out that the offering-stela was the focal-point of 4th-Dynasty mastabas at Giza (p. 94–5). He perhaps should have noted that stelae of this kind were being placed in tombs at Helwan and Saqqara as early as the 2nd Dynasty.

In discussing the shawabti-figure (or shabti, to give it its earliest name), Aldred rightly points out that the purpose of the figure originally was to act as a substitute for the deceased

in the after-life when forced labour was required (p. 128). The *corvée*, however, was used not for 'public works at critical moments during the inundation', but for rehabilitating the fields, the canals, the river-banks and other important public works after the inundation had ended.

The main narrative of the volume ends with two chapters on Egyptian Society and the Egyptian way of life. In the former, Aldred discusses the various classes of Egyptian society from the King to the peasant. This account will be of particular value to the general reader because it provides in succinct and easily readable form reliable short studies of each social class well illustrated with references to texts and representations, the emphasis always being upon the everyday rather than the religious aspect. Religion is reserved for the last chapter and Aldred is wise in avoiding the usual catalogue of gods and descriptions of funerary practices. He concentrates on the essence of Egyptian religious belief and the meaning of that elusive element *Ma'at*.

A very important part of this book is the section of plates and their descriptions. Here the author has succeeded admirably in showing much unfamiliar material as well as many of the obligatory masterpieces. In the descriptions a great deal of additional information is packed. Some of the plates are particularly admirable in that they force the reader to consider afresh well-known objects by concentrating his attention on details. One may mention the group of soldiers from the Assiut model (21), and the panel of the throne of Tut'ankhamūn (48). The construction of the plates, however, may be criticized on three counts. First, the practice of carrying the illustration to the edge of the plate gives the page a cramped look—possibly the opposite effect from that intended. Secondly, the absence of any plate-underlines is inconvenient, although, apparently, unavoidable. The plates are produced in standard form for editions in a number of foreign languages and the method of production precludes the addition of underlines in the appropriate language. Thirdly, the juxtaposition on the same plate, without clear division of objects of disparate size is misleading; e.g. the statues of Hatshepsut (39) and Senenmut (40) illustrated side by side, are shown as if they were of the same height; the former is 77 in. tall, the latter only 28. The case is worse with the statuettes of Amun (70) and Karomama (71).

In the description of the slate bowl (3) Aldred repeats the suggestion that its form was inspired by a metal prototype. The products of this kind in slate are quite unique and are more probably only virtuoso efforts in a material over which the Egyptian craftsman of the time (the Early Dynasties) had complete control. It is doubtful whether he could have produced similar shapes in metal.

This book is very well produced and it is written in a way that should win it a wide public. The text is illustrated liberally with drawings, many from the hand of the author. Again these drawings frequently show unusual things and they admirably supplement the plates. One small slip has been detected in the caption of fig. 21, where the wooden objects illustrated are said to have come 'from a tomb, perhaps of King Djer'. The king in fact is Djet (or Wadji).

One small error has been noted in the description of pl. 52 where the reference to pl. 80 should be to pl. 79.

T. G. H. James

PERVOBYTNAYA TEKHNIKA (Prehistoric Technology). *By* S. A. Semenov. *Materialy i issledovaniya po arkheologii SSSR Nr.* 54, Moscow–Leningrad 1957. *pp.* 240, *with* 106 *text-figures and plates.* 18 *old roubles.*

In the last ten or fifteen years our knowledge of man's earliest past has been advancing so quickly that it may seem to be out of date to review a book which has been published

as early as 1957. As an excuse for doing so I dare only to plead that Semenov's book tackles a long-neglected problem, and that it tackles it in a completely new way.

The title of the book—Prehistoric Technology—might promise more than what one actually finds there: most of the text is devoted to the flint and ground stone industry, a smaller part to the bones. Other parts, especially on the pottery and the early metallurgy, are presumably intended to follow.

The novelty introduced by Semenov into archaeology is not the theme itself. This has not been a virgin soil for years, and in fact some of the methods were well known to the first archaeological generations. It was notably the method of answering the ever present question 'what was it for' by studying how objects similar to the prehistoric ones were used by primitive peoples of the recent past. But such studies were not satisfactory enough, and they were soon replaced by efforts to grapple with the problem by reproducing prehistoric objects through experiment. This has been a sound trend as there is no doubt that we do not know anything unless we are able to reproduce it. Semenov takes full advantage of both these methods throughout his book, but at the same time he draws attention to the fact that they are indirect and thus cannot be decisive. It is for this reason that he introduces an entirely new technique of studying prehistoric tools and other objects. This method is based on the assumption that working by tools leaves specific traces on their surface, and from these the kind of work may be inferred which had been performed by them. Such traces are, however, so trifling that they cannot be usually recovered otherwise than by using a microscope combined with special procedures of making the flint less translucent.

At this point, some of us may exclaim that this is nothing new under the sun. Something like this method has been used before, and some knowledge has been obtained through it, e.g. in the case of the 'glossed flints' (ANTIQUITY, IV, 1930, 184; and IX, 1935, 62). But as far as I know, nobody has as yet used the traces of work on prehistoric tools for reconstructing their functions to such an extent and so consistently as Semenov. Reading his book we are often more than surprised by how many important traits we did not see on the tools, our principal source of evidence for reconstructing the prehistoric past. Palaeolithic blades, often unretouched, appear under the microscope full of glassed patches, pits and furrows whose distribution, forms, direction, etc., are indicative of whether they were used as butchers' knives, wood-working implements or later as sickles. At the same time they may tell us whether the tools were used with or without a handle, and how they were kept in the hand.

To give only some of Semenov's results I mention here that a Mousterian scraper from Stalingrad has been recognized to have served for working hides thus proving the hide-working industry in the Lower (or Middle) Palaeolithic Period. The single-shouldered points (*points-à-cran*) of the Ukrainian Upper Palaeolithic (Gravettian) culture have been identified as having served as both javelin points and efficient hunters' knives used without a handle. A core implement of the same culture, found at Kostenki I has been recognized as a true axe, presumably for dividing mammoth tusks. By this method, too, Semenov easily determines if ground (Neolithic) 'axes' were real axes or if they were rather mounted as adzes.

The same method applied to bones reveals that some Upper Palaeolithic bones were used as polishing implements in the hide working industry, others were provided with a handle and served as hoes. Still others, this time found on the Tripolye settlements, once polished the famous Neolithic pottery. Certain bones from the Greek colony Olbia proved to be rasps used, in combination with sand, for dressing stone slabs. It should be noted that Semenov, studying the early Eskimo (Ipiutak) bone industry by his method, concluded

that it must have been worked by means of metal (iron) burins, and these were later actually found by both the Soviet and American archaeologists.

These are but several of the results achieved by Semenov through his microscope. Some of them are not very surprising, as similar functions for the implements could have been supposed also by other methods. But the important difference is that by other methods it could have just been *supposed*, whereas by observing the microscopic traces of work the function could have been *proved*.

At that, this method seems to be very promising in regard to the Lower and Middle Palaeolithic industries, for which we possess no ethnographic analogies, and which are so simple that experiments are of no help. And yet to know better the earliest tools is our foremost task, as they are the almost only survivors of their time. But here intervenes a considerable difficulty: just these earliest industries are mostly found under such conditions, that their surface is covered by a thick patina layer, and they are often rolled to such a degree that all the traces of the early work disappear. Yet under special circumstances the traces may survive and it is up to us not to let them lie fallow and to use all the methods possible to study them properly.

This has still another aspect. Are the conditions of the excavations, the washing techniques and museum treatment always careful enough to eliminate secondary traces of work?

Semenov is well aware of the historical importance of his studies. The last chapter of his book is full of highly interesting observations on the evolution of stone and bone tools. These cannot be followed here, but at least a point should be made. The typologies which we have up to now reconstructed are often quite formal. We name all the blades with apparent burin flakes the burins. But Semenov rightly points out that on the one hand the so-called burin flakes were often used only to blunt the edge of a blade, and on the other hand pieces without them were actually used as burins. This one is clear, but unfortunately not the only one example of our formalistic approach to the archaeological classification. It is not possible to get much further by such a method, which strongly reminds us of studying cats by counting their hairs. Using all the classical ways of research plus Semenov's microscope we should try to create a kind of *historical typology*, based on what the tools meant for their creators, not what they appear to be to us modern men.

Semenov's book will certainly become indispensable for all those who are deeply interested in the problems of the Stone Age. He has made the first step, and we now should follow him by checking his methods on masses of finds from different countries.

EVZEN NEUSTUPNY

SUMER. *By* A. PARROT, *with a preface by* A. MALRAUX ('The Arts of Mankind', edited by A. Malraux and G. Salles). Translated from the French by S. Gilbert and J. Emmons. *Thames and Hudson*, 1960. *pp.* $l + 397, 467$ *illustrations*. £6 6*s*.

This volume, the first of a French series on the Arts of Mankind, is devoted to the history of art in Mesopotamia and Western Iran from the earliest settlements to the 12th century B.C. Its scope is therefore far wider than the title suggests. Its purpose is to present the antiquities of Mesopotamia to the art historian as 'authentic works of art in their own right, not just "museum pieces" ', and to indicate the contribution of Sumerian civilization to the artistic development of mankind. Architecture, stone and metal sculpture and the minor arts are lavishly illustrated by plates of outstanding quality, so that even those familiar with the monuments may see them with a fresh eye. The authors seek to interpret this art in terms of the peoples and historical events contributing to its development and the religious purpose which it served. The works illustrated are described in detail and where

possible the significance of their themes is explained. Some account is also given of the history and problems of archaeological discovery in Mesopotamia.

This is a useful introduction to the subject which, while primarily intended to inform the layman, has much of interest to offer to the specialist in Near Eastern archaeology. It is, however, to be regretted that certain historical problems, notably the origin of the Sumerians and their relationship to the Semites, are much oversimplified. For example, M. Parrot asserts categorically that the Sumerians, having come from somewhere beyond the Caspian, overran southern Iraq at the end of the 'Ubaid period and brought with them a culture far in advance of that of the original inhabitants. Surely the reader should be informed that the questions as to when the Sumerians first settled in the country and whence they came, if they were indeed immigrants, are far from final solution. Whether the Uruk period represents a sudden and drastic break in culture is also a matter of debate. The statement that it is marked by 'an abrupt disappearance of painted ceramics in all southern Mesopotamia' is certainly at variance with the archaeological evidence which shows that the replacement of painted 'Ubaid by plain Uruk wares was a gradual and somewhat spasmodic process. Certainly the Uruk-Jamdat Naṣr Age was one of remarkable and rapid progress but, apart from the pottery, what sudden technological or artistic advances can be associated with its beginning? The major innovations of writing, sculpture and cylinder seals appear only in its later phases and it is possible to explain them in terms of normal cultural development. Many of its achievements, especially temple architecture, were organically related to the preceding 'Ubaid period. Even if the introduction of different pottery should mark the arrival of fresh people, it is arguable that the Uruk civilization as a whole was the culmination of developments which can be traced back to the earliest settlements. In his account of the Akkadian period M. Parrot lays greater stress on the anti-Sumerian policy of the Semitic kings than the evidence appears to warrant. What grounds are there, for example, for the suggestion that the rise to power of Sargon was due to a reaction against the despotic rule of the Sumerians under Lugalzaggisi, or for the assertion that the Sumerians were 'not displeased' at the Gutian invasion since it freed them from their Semitic overlords? It is certainly an exaggeration to speak of the Sumerians as 'enslaved' by their Akkadian rulers. A small slip to be noted (p. 169) is that Sargon's father is described as a water carrier. This was the occupation of his foster-parent. As he himself says in *The Legend of Sargon*, 'my father I knew not'.

The highest praise is due to the publishers for the care and thought they have devoted to the production of the book. In addition to the excellent plates, many in colour, the reader is greatly assisted by clear architectural plans and maps, comparative tables, a glossarial index and a classified and annotated bibliography. Where such riches are offered, it is perhaps ungracious to mention omissions. Nevertheless it may be regretted that the exquisite animal amulets of the Jamdat Naṣr and Early Dynastic periods and the jewellery from the Ur cemetery are unrepresented.

J. M. Munn-Rankin

UR IN RETROSPECT; In memory of Sir C. Leonard Woolley. IRAQ, Volume XXII, 1960. *Edited by* M. E. L. Mallowan and D. J. Wiseman. *British School of Archaeology in Iraq. pp.* ii + 248, xxxi *plates*, 14 *text figures*. 4 *gns*.

The excavations at Ur were unquestionably the greatest of Sir Leonard Woolley's achievements. In twelve seasons of work, he established a remarkable succession of cities spanning well over four millennia. Of all his discoveries the revelation of Sumerian civilization must rank as the most outstanding and spectacular, but information of the greatest importance was also obtained for later periods. Since the excavation closed in 1934, however, progress

in research and the investigation of other sites have greatly elucidated and amplified the Ur finds. As a tribute to his memory, the editors of *Iraq* have therefore devoted this volume to the discoveries at Ur in the light of recent research. Twenty-five scholars, whose work has been particularly connected with the periods represented at that site, have contributed to it. The wide range of topics covered bears eloquent testimony to the greatness of Woolley's contribution to our knowledge of Mesopotamian civilization.

The earliest, 'Ubaid culture of the 5th millennium B.C., first identified by Woolley's excavations at Tell al 'Ubaid and Ur, is reassessed in the light of subsequent finds, notably at Eridu and Tell 'Uqair, in articles by Seton Lloyd and J. Oates. The Early Dynastic period at Ur is the subject of several contributions. M. E. L. Mallowan discusses the adjustments now required in Woolley's chronology and sequences and also recalls personal memories of the Ur expedition and its leader. In connection with the human sacrifices found in the royal tombs, C. J. Gadd enquires into the historical significance of similar practices in First Dynasty Egypt, while S. N. Kramer examines the textual evidence for Sumerian ideas about the after-life. For the period of the Third Dynasty of Ur, H. J. Lenzen gives a detailed architectural analysis of the sacred areas of Ur and Uruk and A. Falkenstein and A. Goetze each contribute texts relating to Šulgi. E. Sollberger catalogues and offers notes on the early inscriptions from Al 'Ubaid and Ur and two incantation texts from Ur are published for the first time by C. J. Gadd and O. R. Gurney respectively. T. Jacobsen reports on the investigations of the Oriental Institute into the canal system of Babylonia, with special reference to Ur, and H. Helbaek considers the ecological effects of irrigation in ancient Mesopotamia. The location of the biblical Ur of the Chaldees and of Bad-tibira are the subject of articles by H. W. Saggs and V. E. Crawford. The studies of individual objects from Ur and of finds from other sites which have connections with the Ur material are too numerous to list individually.

J. M. MUNN-RANKIN

A MATTER OF TIME. An archaeological survey of the river gravels of England prepared by the Royal Commission on Historical Monuments (England). *London: Her Majesty's Stationery Office*, 1960. *pp.* 64, 10 *figures and* 12 *plates.* 10*s.* 6*d.*

The editor has already (ANTIQUITY, 1960, 241–3) congratulated the Royal Commission on its timely production of a well-planned and attractive guide to archaeological sites revealed, and threatened, on the river gravels of England. Not only does the format show commendable journalistic initiative, but the impressive apparatus of tables, maps, and bibliography indicates a precise concern for the needs of the student working in detail, in the library and the field, on the development of settlement on the gravels. In the hope that this will be only the first of similar moderately priced volumes stimulating and guiding urgently needed field projects, I offer a few consumer-comments.

I foresee a new parlour game, 'Hunt the number', to be played by local archaeological groups on dull winter evenings. Objective—to plan a ground survey of settlement sites on gravels in your chosen area. Starting-point—the 'Appendix of Settlement Sites in the Ouse, Trent and Welland Valleys'. Bonus, for those concerned with one stretch of the Welland valley—a series of large-scale maps, figs. 6 to 10. Example of play: Find third site listed under 'Maxey'. Clue—'See fig 6, no. 54'. Hidden trap—there are five independent series of site numbers, by parishes, on fig. 6. Search between Maxey 53 and 55. Not there. 'Grid reference TF 123082'. But *the maps are not gridded* (why not?). Race between contestant who ransacks map store for stack of gridded sheets which he can correlate with fig. 6, and opponent who meanwhile has located 54, north-west of 56. (There are 111 sites to look through in Maxey.)

Or another variant, 'Chase the evidence'. Objective—to collect details of one particular site. Example of play: Find out all you can about enclosure-group shown in pl. 4*a*. 'Notes on the Plates' refers you to Maxey, Northamptonshire and fig. 6, but supplies no site number (except of the adjacent pit-alignment), grid reference, or details of photographs. From the table of Pit-alignments you may get the grid references of the two alignments in Maxey, but not their site-numbers, so you cannot tell which is the one on the photograph. A close and skilful examination of the Maxey map will eventually identify the layout of the photograph with that of site 17. From the Appendix you can now find the grid reference[1] of the heterogenous assemblage 17–21, a description, and cross references to plates (6 shows a circle group a quarter of a mile to the west). Here, and from pages 14, 28, and 36, found by reading right through the text, you learn that the site was strewn with 'late Roman' or 'fourth century' pottery, but not who found it or when. The text descriptions give no grid-references or site-numbers, so only the closest attention will identify them with this site.

The really interested field worker needs full details of previous field work,[2] and catalogue references to air photographs, like those given in the table of Pit-alignments, but not in the tables of Enclosures, Circles, Cursuses, the Appendix, nor, most strangely, the List of Plates. Only from the Acknowledgments can one discover even who took the photographs. To be as useful as they look, plates, maps, text and tables need to be linked by a consistent system of cross-references, with one system of site-reference, not two, preferably a grid reference, and gridded maps.

Those interested in the vicissitudes of settlement and land-holding during the Dark Ages will examine the detailed maps and plates carefully before deciding whether or not the pit-alignments on them *do* 'mark out land-holdings essentially *un*connected with the Roman layout', especially after reading the deduction, a few lines earlier, that one complex of alignments was 'contemporary with, or a little later than the Roman period'. It is for such presentation of evidence from which we can draw our own conclusions that we are most grateful to this survey. It should serve as a challenge to intensive ground surveys, from which alone we can ever gather the rapidly perishing evidence of dates and densities for this continuous settlement on the gravels.

SYLVIA HALLAM

[1] It is not possible to find the grid references of all the photographs. Try pl. 3*a*.

[2] Why state that Dr St Joseph has examined on the ground nearly all the Settlement Sites listed in the Appendix, if the result is not disclosed?

THE EARLY CHRISTIANS. *By* MICHAEL GOUGH. (Volume 19 in the 'Ancient Peoples and Places' series.) *London: Thames and Hudson*, 1960. *pp.* 211, *and* 48 *pages of photogravure illustrations*, 31 *line drawings*, 1 *map*. 30*s*.

In this attractive book the author, who is the new Director of the British Institute of Archaeology at Ankara, has written a well-documented and beautifully illustrated survey of Christian art and archaeology down to the end of the reign of Justinian. He has divided his study at the Edict of Milan, and set both the pre- and post-Constantinian periods in their context of Church history and doctrine.

This method has enabled him to explain the symbolism of early Christian iconography in terms of beliefs which have already been outlined. Plans, illustrations and text all form part of a single narrative. While, inevitably, with the publication of van der Meer's and Mohrmann's *Atlas of the Early Christian World* only three years old, much of the material has been discussed before, the author places his own stamp upon it, and where he is dealing with his chosen field of south-eastern Turkey in the 5th and 6th centuries his mastery is evident. For the earlier period, much of the reader's attention is focussed on Christianity

in Rome, illustrated by the art of the Catacombs and the cult of SS. Peter and Paul, but there is also a valuable discussion of the Christian art of Dura-Europos and the rustic symbolism of the Montanist inscriptions of the Tembris valley. We see the early symbols of hope and redemption, the wine, the bread, the fish, the Christian *orante* and Christ as Good Shepherd. How this vigorous but immature art developed into the unsurpassed greatness of San Apollinare and Santa Sophia is clearly and learnedly set out. This book, even if one may think some of the author's analogies fanciful (cf. p. 57), is a pleasure to read.

The problem in a work of this nature is selection, how to present a balanced description of the life and art of the early Christians without many footnotes and in a limited space. The author rightly spends time on the Classical background, and emphasizes the influence of native and Oriental religion, including Mithraism, on Christian art. But one essential influence, namely the Hellenistic-Jewish synagogue has been neglected. Without an assessment of this legacy, Christian history down to the time of Origen is meaningless. It was not only 'contemporary pagans' (p. 38) who regarded Christianity as an offshoot of Judaism. The well-known texts of Origen, *Contra Celsum*, II, 1, Tertullian, *Adversus Marcionem*, IV, 8 (*Nos Iudaei Nazarenos appellant*) and ch. 1 of the Gnostic *Apocryphon Ioannis*, suggest that the parentage was not lost on the Jews themselves from areas as widely separated as Palestine, Carthage and Alexandria in the period 150–200. Indeed, the internal problems which were worrying Christians at this period, such as the precise method of dating Easter, whether one fasted on Mondays and Thursdays or Wednesdays and Fridays, the role to be attributed to Torah, or the apocalyptic view of contemporary society, can only be understood within the framework of the synagogue. The organization and liturgy of the Church was also derived from its Jewish contemporary, the difference being that, for the Christian the Final Age had already begun, while for the Jew it was still to come. *Both* were ardently proselytising religions down to 200.

Naturally, all this has left a profound imprint on early Christian art and architecture. At times, the author would appear to be ready to concede this, but his discussion whether the *pulpitum* of Cyprian's church in Carthage *c.* 250 might be derived from the reading desk of a Jewish synagogue does not broaden out into a more general inquiry whether the apsed synagogue may have been the prototype of the later basilican church. After all, Cyprian, Origen and Paul of Samosata were preaching to crowds in churches half a century before the Baths of Maxentius were thought of. It is surely not unreasonable to look for causes of the otherwise puzzling similarities between basilicas in North Africa and Syria in a common Jewish prototype.

The narrative sections demanded extreme skill in compressing material without distortion. It would have been better perhaps to have given space to two great figures such as Origen for the East and Cyprian for the West and shown how the various movements of orthodoxy and dissent in the 4th and 5th centuries could be related to the different emphases placed on aspects of their teaching. As it is, the treatment of dissent tends to be episodic, and this is exaggerated by failure to include examples of the popular Christian art of the African Donatists (see ANTIQUITY, 1942, 342–52) and the Coptic Monophysites. Indeed, far from the Church's progress being 'inexorable' (p. 206), wherever the regional dissenting Churches were persecuted by the orthodox, Christianity itself has withered away; and a glance at a map of the southern and eastern shores of the Mediterranean is sufficient proof of this. This is a problem which anyone interested in the early Church must seek to answer, and clearly the answer is not to be found in any special plea in favour of uniformity.

But allowing for these criticisms, this work is highly to be recommended.

In a concise and extremely interesting manner it tells the general reader just how much

the student of the early Church owes to the archaeologist. It shows too, the enormous amount that remains to be done. Much salvage work is already being carried out by the British Institutes of Archaeology, but the steady growth of population is endangering more and more sites. Here is something on which the Institutes and Universities should be working together. Plans should be laid now (and funds earmarked) for emergency salvage work, for time may be running short.

Regrettably, a number of factual errors have slipped in:

p. 46: Clement was never bishop of Alexandria, and he was dead some 35 years before the Decian persecution.

p. 47: The Novatianist schism followed the Decian persecution immediately and not 'some years later'.

p. 108: Licinius was not strangled in 323. He was very much alive at the battle of Chrysopolis on 18 September 324, and probably survived until 326.

p. 108: Maximian did not trouble the Church 'for some years' after the Edict of Milan in 313. He died of disease at Tarsus in the summer of that year.

p. 110: For great schism of 954, *read* 1054?

p. 193: Surely 568 (the reign of Justin II) is too late for the mosaics of the Basilica St Apollinare Nuovo (*read* 528?)?

p. 33: What is the evidence for Diocletian emerging from retirement in 307 to proclaim Mithras as patron of the Empire?

p. 206: How could Martin of Tours, died 397, consecrate S. Patrick (born 390) bishop?

W. H. C. FREND

INDIAN LIFE IN THE UPPER GREAT LAKES 11,000 B.C. to A.D. 1800. *By* GEORGE IRVING QUIMBY. *The University of Chicago Press*, 1960. *pp.* xv + 182, 75 *figures* (*photographs, maps and drawings*). $5·95.

Even if the author of this book were not well known in American archaeological circles, his competence to write it would be sufficiently attested by his membership of the staff of the Chicago Natural History Museum and his part in that admirable synthesis of North American archaeology *Indians before Columbus*. He says that the book was primarily written for the interested layman and the student rather than the specialist, although he hopes that the specialist may find some of the data and ideas useful. I believe that he will, but I do not know how he will receive the introduction of the term Aqua-Plano to designate a lacustrine variety of the post-Folsom hunting tradition sometimes called Plano in the West, which leads Quimby to lump a wide variety of large projectile points together under this label.

The area covered is that immediately surrounding Lakes Superior, Michigan and Huron. Roughly two thirds deal with archaeology from the time when the retreat of the Quaternary ice sheets (how I dislike the term 'Glacial Ice'!) first permitted settlement up to A.D. 1600, and the remainder with the Indian tribes who subsequently lived and moved there. The book is packed with reliable, up-to-date information, and is well illustrated by photographs and numerous sketch maps showing the position of the ice front, the distribution of land and water, and the main vegetation zones at different stages. The reader cannot fail to be struck by the amount of geological work, vital to the archaeology, which has gone into the unravelling of the extremely complex topographical changes brought about by movements of ice and isostatic changes of height before the lakes took their present form about 500 B.C. It is noteworthy that the author holds to the view that the Boreal Culture of 5000 to 500 B.C., as well as the Early Woodland pottery tradition which started in the Lakes about 500 B.C., were connected with counterparts in Eurasia, in spite

of that elusiveness of the actual links which causes hesitation to some archaeologists. A fact which gives food for thought is the number of different tribes living side by side in this limited area in historic times, and one wonders how many of these divisions would show up in the archaeological record.

The author says that 'in effect, each chapter was treated as an independent project in terms of both research and writing'. This explains rather a tiresome stylistic defect in the book, and it has resulted in a good deal of unnecessary repetition. Thus Chapter 3, The Archaeology of Environment, had much better have been incorporated in the subsequent chapters, since nearly all the information in it is repeated in them. On p. 34 'an earlier stage' is referred to as though it had not been mentioned before, although it is the subject of the previous chapter. Similarly on p. 59 we read 'During this period there were two additional large lakes in the region'. One of these, Lake Agassiz, had already been mentioned nine times, and the other, Lake Ojibwa-Barlow, four times, once at least in practically identical terms. The slight extra trouble needed to make the book into a really coherent whole would have been well worth while.

A book of this sort is bound to make easier reading for those who are familiar with the region, and it may be surmised that the intelligent amateurs at whom it is directed will mainly be drawn from the Great Lake region. Outside the United States and Canada it will be essential reading for any archaeologist who is trying in some measure to keep abreast of developments in North America. Knowledge of American archaeology has increased to such an extent in recent years that it is probably too much to expect an up-to-date edition of *Indians before Columbus*, but a series of regional studies on the lines of this one would be a very welcome substitute.

G. H. S. Bushnell

EGYPT OF THE PHARAOHS, AN INTRODUCTION. *By* Sir Alan Gardiner. *Clarendon Press: Oxford University Press*, 1961. *pp.* xx + 461, 22 *plates*, 17 *figures*, 3 *maps*. 35*s*.

Sir Alan Gardiner, the doyen of British Egyptologists and a generous instigator and patron of Egyptological enterprises, in his eighty-second year has produced what he pessimistically refers to as his probable swan-song. This is not, as might be supposed, one of his monumentally erudite works on some specialist aspect of his subject, with the full philological commentary dear to his grammatical mind. It is something less analytical, a synthesis in fact of much of his thought and observations not only upon Ancient Egypt and its history, but also upon the nature and progress of Egyptology itself. As the reflexions of a keen, fecund, and mature mind, this work cannot fail to be of the utmost interest and stimulus to his colleagues who will be as eager to examine his arrangement of vague or incomplete data to form a coherent pattern as they will to consider his suggestions for the solution of thorny problems. His interpretations of such disputed events as the unification of Egypt at the dawn of history, in which he tends to support the views of Sethe, the end of Dyn. xix and the fall of the Ramessides, for instance, are deserving of the closest study. Less valuable, perhaps, is his discussion of co-regency, especially during the 'Amarna period where he regards the 'hypothesis' of the well-attested co-regency of Akhenaten and Smenkhkarē' as 'doubtful', yet perversely accepts the co-regency of Tut'ankhamūn and Ay on far less solid grounds. Many of his *obiter dicta*, such as the discussion of the phrase 'God's Land' (p. 138), are suggestive and open up new vistas of research. On the other hand, some of his evaluations, especially those dealing with the marital relationships of the Pharaohs, are made from a modern European standpoint which the social anthropologist has learnt to put in a better perspective by a willing suspension of distaste.

But Gardiner has not primarily been concerned with his colleagues or the advanced student. He has addressed himself to a wider public and particularly the beginner. Here he has introduced right from the start a serious deterrent in the form of a new and personal system of nomenclature which the learner will be perplexed to find is not followed by the other authorities to whom he is referred *passim*. In some of the transliterations, Zanakht for instance, letters are employed which have no mention in the alphabetic lists recommended in the various editions of Sir Alan's authoritative *Grammar*. The author's justification (p. 52–53) for this adventurous, but perhaps ill-advised, decision is that among other things it demonstrates the progressive nature of Egyptology. If this novel system does in fact bring home to the reader the necessity for accepting nothing on trust in Egyptology, not even perhaps the opinions of Sir Alan himself (especially those expressed in Chapter IX), this reviewer for one believes that a valuable lesson will have been taught.

In reading this stimulating work, the reviewer has been compelled now and again to differ from the author in his assessment of this or that feature or episode; but such judgments can only be matters of taste or opinion. The following observations, however, are offered in the interests of that strict accuracy on which Gardiner has always rightly insisted.

Plate IV. To speak of the 'pyramid-temple' in connection with the vast complex of buildings that forms the Step Pyramid of Djoser is misleading. The illustration in any case shows the rebuilt entrance-port in the enclosure wall.

p. 140. The statement that Petrie found two colossal seated statues of Ammenemes III looking over the lake at Biyahmu gives a wrong impression. What he found was a greatly devastated monument which he re-constructed from the scantiest evidence. Readers of his *Hawara, Biahmu and Arsinoe*, pp. 53–6 will find that all he was able to recover of the statues were a nose, some 'drapery' and scattered fragments of the thrones. From these he calculated their probable form and height.

p. 197. The gold 'coupe de Thoutii' in the Louvre is here referred to as a 'plate'. Since it has a deep, vertical upturned rim, 'dish' or 'bowl' would be a more accurate description.

p. 207. Since Petrie's initial statement that the pavement which he found at 'Amarna was painted while the plaster was wet and while it could still be moved by a brush, the claim has been repeated that the Egyptians knew the technique of true fresco-painting. This, however, has been denied by Lucas and the evidence is on his side. The fragments of painted pavements have more the appearance of fresco because, unlike wall-paintings, they have been exposed to the elements including chance rain-storms.

p. 220. From the moment of their discovery of the sculptors' quarters at 'Amarna, the Germans have described them as the studios of the sculptor Dḥutmose; and this convenient term of reference has been employed so regularly and widely since, that 'the Master-sculptor Dḥutmose' is now in distinct danger of having the entire designing if not execution of the sculptures found in the area accredited to him. Scharff, however, has pointed out (ANTIQUITY, XI, p. 180) that the name occurs only once incidentally on a small object found on the site; so it is not even certain that Dḥutmose was the owner of the workshop, let alone its chief executant.

p. 222. It is only in the earlier proclamations on the Boundary stelae at 'Amarna that Akhenaten announces precise arrangements for the ultimate burial of his family in the Royal Tomb, but he mentions one daughter only in these contexts, the eldest, Meritaten.

p. 355. The impression is given that in the Late Period it was the reliefs and sculptures of the Old Kingdom only that were nostalgically recalled. In fact, the art style of all past periods of greatness was copied, whether of the Old, Middle or New Kingdoms. In Ptolemaic times even the recent past of the Libyan and Saite Ages inspired a memory of former grandeur.

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